NATURAL PHONOLOGY AND LOANWORD PHONOLOGY (WITH SELECTED EXAMPLES FROM MIAMI CUBAN SPANISH)

By

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A DISSERTATION PRESENTED TO THE GRADUATE COUNCIL
OF THE UNIVERSITY OF FLORIDA
IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE
DEGREE OF DOCTOR OF PHILOSOPHY

UNIVERSITY OF FLORIDA

1976

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DEDICATION

This dissertation is for MARY ANITA who was its inspiration and BOHDAN who in the end made it possible.

ACKNOWLEDGEMENTS

Besides all the expected types of indebtedness surrounding any advanced graduate education, this dissertation in one special sense or another owes some small part of its existence to each of the following institutions and individuals:

My advisors, Bohdan Saciuk and Gary Miller, for remaining always patient and providing so many inspirations along the way. Much of what I know about phonology can be credited directly to Gary Miller; and Bohdan Saciuk has been the kind of teacher, counselor, and personal friend I am not likely, soon if ever, to meet again.

The Graduate Council and the Faculty of the Program in Linguistics, for generously awarding the Graduate Council Fellowship and two Graduate Research Assistantships which have made my studies at the University of Florida financially possible.

The Department of English at George Mason University for a faculty appointment which provided the needed final impetus for my work.

David Stampe, for his early encouragement and for providing me with a topic of relevance. Also, for better or for worse, David's unbroken silence has repeatedly forced me to reach my own unique and hopefully fresh solutions to the problems and issues his theories have inevitably generated.

Julie Lovins, for her painstaking and encouraging comments, her genuine interest, and several letters from "the land of the rising sun."

Richard Rhodes, for his many enthusiastic and pregnant letters

and for the motivations for Chapter Two.

Tracy Terrell, for responding rapidly and generously to my requests for his own papers and works-in-progress on Cuban Spanish -- these works having provided an inspiration as well as a practical and indispensable guide.

Bill Carrillo and Sergio Arnaiz, for all their aid in tracking down several Miami informants.

Joan Hooper, Jorge Guitart, Joseph Matluck, Richard Wojcik, and Jonathan Kaye, for answering all my demanding and sometimes impertinent letters and requests.

Mrs. John B. Whitworth, my mother-in-law, for financial and emotional support when it was most needed and for the gift of a tape recorder.

Mr. and Mrs. Robert Bjarkman, my father and mother, for so many intangible gifts over the years which have helped in some small measure to make me a better scholar and a better teacher.

And Mary Anita, who after everything is said and done is, above all, the one who made it possible.

Somewhat belatedly, I would also like to thank Chauncey Ch'u, whose classes were an enjoyable and satisfying learning experience, whose own time was never too precious to spend a few moments with his inquisitive students, and whose friendship I will treasure for a long time to come as I look back fondly on my Gainesville experience.

TABLE OF CONTENTS

ACKNOWLEDGEMENTS iv

ABSTRACT viii

Part One Introduction to Natural Phonology
CHAPTER ONE BACKGROUND AND ISSUES 2

1.1 Introduction 2
1.2 Claims of Natural Phonology 10
1.2.1 The innateness of natural processes 11
1.2.2 The restrictive nature of phonological processes 12
1.2.3 The revisions of the innate system 13
1.2.4 The natural order of acquisition 14
1.2.5 The internal representations of adult speech 15
1.2.6 The innate residue in adult grammars 16
1.2.7 Naturalness motivations in linguistic change 18
1.2.8 The prominence of natural loanword phonology 20
1.3 Explanation and Performance Models 25
1.4 Remarks on Cuban Spanish and Methodology 40
NOTES 50

Part Two The Processes of Natural Phonology
CHAPTER TWO RULE ORDER AND STAMPE'S NATURAL PHONOLOGY 57

2.1 Preliminary Remarks 57
2.2 Conceptions of Rules and Natural Processes 60
2.2.1 Non-suspendability of natural processes 81
2.2.2 Optionality of natural processes 82
2.2.3 Extremity of natural processes 83
2.2.4 Non-linearity of natural processes 84
2.2.5 Phonetic functioning of natural processes 99
2.2.6 Allophonic influences of natural processes 100
2.3 On Paradigmatic and Syntagmatic Processes 113
NOTES 140

CHAPTER THREE COMPETING MODELS IN NATURAL PHONOLOGY 146

3.1 Explanation in Stampean Natural Phonology 146
3.1.1 Hypotheses in Natural Phonology 150
3.1.2 The uniqueness of Natural Phonology 176
3.1.3 Two cases of reanalysis 188
3.2 Naturalness and Some Notational Variants 196
NOTES 203

CHAPTER FOUR WORD FORMATION AND PHONOTACTICS IN NATURAL PHONOLOGY 209

4.1 Surface and Underlying Constraints 209
4.1.1 The use of morpheme structure conditions 215
4.1.2 The use of surface phonetic constraints 231
4.2 Active and Passive Grammatical Components 240
4.3 On the Reality of Morpholexical Rules 252
NOTES 261

Part Three The Applications of Natural Phonology
CHAPTER FIVE NATURAL PHONOLOGY IN THE MIAMI CUBAN DIALECT 264

5.1 Introduction 264
5.2 Issues in Cuban Phonological Studies 265
5.3 Some Selected Processes in Miami Spanish 286
5.4 Implications of Natural Processes 290
NOTES 301

CHAPTER SIX LOANWORDS AND PHONOLOGICAL STRUCTURES 305

6.1 Structural Implications of Borrowing 305
6.2 Earlier Inadequate Approaches to Borrowing 310
6.2.1 Haugen and Weinreich and Structuralism 311
6.2.2 Chomsky and Halle's Admissibility Theory 314
6.2.3 The "Magnetic Attraction" Hypothesis 317
6.3 The Applications of Natural Loanword Phonology 321
NOTES 331

CHAPTER SEVEN LOANWORD PHONOLOGY IN THE MIAMI CUBAN DIALECT 333

7.1 Extent and Types of Borrowing 333
7.2 Phonological Nativization and Phonological Adoption 348
7.3 Conclusions and Revisions in Natural Loanword Phonology 350
NOTES 362

Part Four Syllabic Phonology
CHAPTER EIGHT SYLLABIC PHONOLOGY AND NATURAL PHONOLOGY 365

8.1 A Theory of Syllabic Phonology 365
8.2 Consonantal Strength Hierarchies 369
8.3 Weakening Chains in Natural Phonology 387
8.4 Strength and Spanish Syllable Structure 393
NOTES 401

APPENDIX 403

REFERENCES 416

BIOGRAPHICAL SKETCH 428

Abstract of Dissertation Presented to the Graduate Council
of the University of Florida in Partial Fulfillment of the Requirements
for the Degree of Doctor of Philosophy

NATURAL PHONOLOGY AND LOANWORD PHONOLOGY
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December, 1976

Chairman: Bohdan Saciuk
Major Department: Linguistics

This dissertation provides inter alia the first extensive application of Natural Phonology proposed by Stampe to an investigation of linguistic borrowing. My predominant purpose is to test out some central claims of Natural Phonology -- a nascent explanatory theory of phonological components in the grammars of natural languages being developed by Stampe and his students from the initial assumption that a phonological system is largely the residue of an "innate" system of phonological processes which are significantly revised by the early activities of language learning. It is demonstrated with these eight chapters that claims made by the natural phonologists draw considerable support, among elsewhere, from findings in loanword phonology: loanwords offer examples corroborating the observations about child language acquisition which so far make up the bulk of Stampe's evidence. It is also the purpose of this work, however, to suggest revisions and amendments at points where Stampe's theory has already proven intractable and insufficient in the face of a large corpus of cross-linguistic data.

Part One is a comprehensive introduction to Natural Phonology. The three chapters of Part Two provide a theoretical background by evaluating some strengths and potential weaknesses of Stampe's pioneer model. Chapter Two clarifies several misconceptions existing in the current literature concerning "rules" versus "natural processes," and on the status of the natural phonemic level of representation which is the relevant level of abstraction in Natural Phonology. Chapter Three outlines some additional hypotheses which are the foundations of Natural Phonology -- the Innateness Hypothesis, the Suppression Hypothesis, the Acquisition Hypothesis, the Phonemic Hypothesis, the Phonetic Change Hypothesis -- and summarizes several supporting arguments which have appeared only in Stampe's unpublished work (circulating preliminary papers) and are likely unfamiliar to many linguists. Chapter Three also takes up differences between Stampe's version and often incongruent competing variations on Natural Phonology and examines the related problematical areas. In Chapter Four a revised Stampean model is suggested as part and parcel of a more comprehensive Natural Process Morphophonology, incorporating within it a separate word formation or morphological component. This revised model is intended to make lucid precisely how Natural Phonology might more adequately account for both underlying and surface level constraints, which together operate on both the native and "exceptional" (foreign) phonological forms.

Discussion in Part Three of this study is devoted to the native phonology and loanword phonology of Miami Cuban Spanish as these are revealed through apparatus of Natural Phonology. Whereas Part Two is concerned largely with defining the concept and role of processes in Natural Phonology, Part Three emphasizes actual applications of the

theory. More specifically, Part Three explores the implications of principles of Natural Phonology for the analysis and explanation of loanword data and in the process supports and illustrates claims of the original Stampean model of Natural Phonology as well as the revised model rendered in Chapter Four. This integral part of the dissertation (especially Sections 7.1 through 7.3) extends a series of claims made by Julie Lovins (1973 University of Chicago Dissertation) that loanword phonology essentially is natural phonology and that phonological systems are perhaps actually best revealed through active processes in linguistic borrowing. Evidence for the dominant role of natural processes in loanword assimilation derives from tapes of bilingual as well as nearly monolingual speakers of the Miami Cuban dialect of Spanish, a dialect which is an unparalleled source of contact borrowing in Spanish phonological studies.

Finally, Part Four demonstrates how fundamental theses of Natural Phonology will conspire along with dialect-specific examples from borrowing to modify certain basic assumptions in the literature about the role of syllable structure in an explanatory phonological grammar.

Part One

Introduction to Natural Phonology

CHAPTER ONE

BACKGROUND AND ISSUES

> Natural Phonology is essentially a continuous search for the optimal grammar which is consistent not only with language-specific data but also with known properties of human languages in general.
> -- Matthew Y. Chen

1.1 Introduction

A primary justification for this present dissertation is the paucity of previous conclusive work on loanword phonology, while another is the incomplete status and relative unavailability of David Stampe's own pioneer efforts in developing the model of Natural Phonology. With his few articles (Stampe 1969, 1972, 1973c) and circulating papers (Stampe 1968, 1973b), Stampe provides limited examples from outside of child language and fewer still from outside English. And though it is his own assumption (see especially Stampe 1969, page 451, footnote 4) that loanword phonology provides the crucial testing grounds for any phonetically-based and/or phonologically-based model, very little has yet been said by Stampe himself or others about loanwords of specific dialects in any of the available accounts of Natural Phonology.[1]

The theory of Natural Phonology which Stampe (1969) proposes as an essential modification of Chomskyan "generative" phonology is based almost exclusively on certain empirically testable hypotheses about the universal physiological basis of principles governing language acquisition and language change. There is the hypothesis, for example, that the phonological system of a language is an innate system of

physiologically-induced articulatory processes, as well as the connected hypothesis that language acquisition is largely a matter of randomly suppressing these innate universal processes.

These hypotheses are first outlined in Stampe's earliest publication (Stampe 1969) and more recently expanded with finer illustration in his seminal doctoral dissertation (Stampe 1973a). They have become as well the cornerstones for publications on both child language acquisition and linguistic borrowing authored by a small force of Stampe's students and others prominently supporting Natural Phonology (viz. Lovins, Edwards, Ohso, Wojcik, Bjarkman). Yet it still remains that the literature on Natural Phonology is not at all extensive. An almost complete listing of the works devoted to advocating Stampe's theories would include no more, perhaps, than the following dozen or so entries: Darden 1971; Edwards 1970; Hutcheson 1973; Lovins 1973, 1974a, 1974b; Donegan Miller 1972a, 1972b, 1973a, 1973b; Nessly 1973; Ohso 1971; Rhodes 1972, 1973, 1974; and most recently Wojcik 1976. Also there are earliest drafts of sections of this dissertation, which have appeared as Bjarkman 1974a, 1974b, 1975, 1976. Articles or monographs most severely critical of Stampe's claims, on the other hand, would especially be the following: Dressler 1974; Hooper 1975; Lee and Howard 1974; Miller 1975; Ohala 1974; and a dissertation by Stoel (1974). Of these latter works, those by Dressler, Lee and Howard, and Ohala have all simultaneously appeared in the single Chicago Linguistic Society volume entitled Papers from the Parasession on Natural Phonology (cf. Bruck et al. 1974).

There is, of course, a plethora of brief references to Stampe's theories throughout numerous papers and monographs on child language (e.g. Ingram 1974 and Golick 1974). But it is not at all inappropriate

to observe at this point that exceptionally little is offered within this second list of papers to supplant or even noticeably weaken crucial assunptions of the natural phonologists. To date, the theory has been superficially praised in some quarters while being altogether ignored in others.[2] But Natural Phonology has surprisingly not been very seriously challenged even by its severest critics, nor has it yet been very adequately tested out by even its most enthusiastic proponents.

This dissertation will undertake answering some of the largely superficial objections to Stampe's theory, as well as most of the more substantive criticisms of Natural Phonology. I will both support the model where it offers elegant solutions to long-standing problems in phonological theory and at the same time offer several amendments where there are inevitable shortcomings in its present version. My approach is one designed to combine a double purpose: (1) of explicating Natural Phonology in some of its more elusive aspects and (2) of exploring its pragmatic applications to what has proven to be a sparsely attended field of loanword theory.

The four chapters in Parts One and Two provide a sketch and an expansion of the present model of Natural Phonology. Chapter Two, especially, clarifies Stampe's distinctions between the basic innate "natural processes" (i.e. those substitutions which are mentalistic and physiological in their motivations) and the acquired "rules" (i.e. the standard-type phonological rules of traditional generative phonology) and establishes a working conception of the "natural phonemic level" postulated for Stampe's model. Some theoretical arguments against a level of phonemic analysis and also against Stampe's proposal of paradigmatic or context-free natural processes are dismissed

as being without sufficient foundation. Chapter Three takes up further evidence for the Phonemic Hypothesis and other crucial hypotheses of Natural Phonology. A distinction is made between the version of Natural Phonology proposed by Stampe and other notational variants, such as Vennemann's Natural Generative Phonology (Vennemann 1971, Hooper 1973) and Schane's notions of naturalness (Schane 1973). An overview is also presented of some troublesome inconsistencies in the theory as Stampe formulates it to date. Stampean phonemics are compared to classical phonemics and it is claimed that a level of phonemic representation is not only motivated empirically but also capable of withstanding effectively each of Halle's and Chomsky's famous arguments against the phoneme as a viable linguistic concept. These first several chapters are, then, a necessary summary and explication of the evidence and assumptions underlying a more or less still tentative model for Natural Phonology.

Chapter Four amends Stampe's view of a phonological component to allow additionally the rules of morphology apparently needed to explain some of the evidence from loanword phonology as well as to account for other notable deficiencies in current generative theories. The type of separate morphological component proposed is similar to one originally envisioned by Halle (see also Aronoff 1976 and Harris 1974) with a list of morphemes, rules of word formation, and a lexical exception filter. It suggests as well a necessary division between an Active Grammar, which comprises the syntactic and phonological components through which all new derivations automatically pass, and a Passive Grammar, which is the paradigmatic processes and morphological component which are only irregularly operable in the adult version of the language

mechanism.

My contention in Chapter Four is also that recent arguments for surface phonetic constraints (the SPC's) as an alternative to morpheme structure conditions (the MSC's) in generative grammar represent a false dichotomy between actual surface phonotactics and the underlying morphophonemic rules. That such underlying constraints on "morpheme" structure do not now seem to be a valid theoretical consideration (such restrictions in Spanish and apparently in most languages are on "words" not "morphemes") does not altogether preclude "lexical" as well as "phonetic" constraints in a generative grammar. It is proposed in this chapter that what have traditionally been referred to as SPC's are captured within a revised Stampean model in terms of active applications of the syntagmatic natural processes and are not at all some kind of "static conditions" in the grammar. Similarly, underlying restrictions (formerly the MSC's) must be taken not strictly as Stampe proposes (as elusive paradigmatic processes which supposedly are a type of filter on the lexicon: i.e. the process, for example, which eliminates all nasalized vowels from the lexicon of English or the one blocking underlying velar nasals in this same language), but rather as a concert of syntagmatic processes acting in conjunction with the templates which are the word formation rules of what we will call the "passive" morphological component.

It is shown, finally, that recent proposals in Sommerstein 1974, Shibatani 1973, Clayton 1976, and elsewhere for phonotactically motivated rules as a modification of the standard generative theory are valid only if the questionable assumption is made that all phonological substitutions are of an identical teleology. Natural Phonology, with its

basic distinction between innate processes and acquired rules, of course denies this overly strong assumption.

Discussion in Part Three is devoted to the native phonology and loanword phonology of Miami Cuban Spanish, as these are revealed through apparatus of Natural Phonology. Data is drawn from quality cassette tapes of twelve primary informants, obtained in formal or semi-formal interview sessions, as well as from fifteen supplemental tapes (about ten hours) of Spanish radio programming and unstructured "free-speech" situations.

Chapter Five examines identifying characteristics of the Miami Cuban dialect, e.g. random Velar Nasal Intrusion (velarization of syllable-final nasals), voicing of voiceless intervocalic stops, spreading of the related obstruent spirantizing process (surfacing of voiced stops as spirants), and final consonant deletions. It is demonstrated, for example, that the rampant phenomenon of Velar Nasal Intrusion by which most final nasals are realized in systematic fashion as velars is a progressive-type assimilation involving preceding vowels and hence a process of general weakening related to most other weakening processes characteristic of this dialect. The argument is developed that evidence from loanword phonology and principles of Natural Phonology combine to suggest also that predominance of spirantized voiced obstruents results from the spreading of a natural process characteristic of an "easy-articulation" dialect and not from any phonemic restructuring of underlying forms (as implied in Hammond 1975, 1976). Chapter Six, in turn, assesses past failures to deal insightfully with data from linguistic borrowing under a variety of theoretical approaches and suggests, by contrast, the full explanatory powers characterizing what Lovins (1974b)

has labelled Natural Loanword Phonology.

Chapter Seven is a central chapter which extends the explanatory powers of Natural Loanword Phonology to an examination of the precise role of processes, opposed to learned rules, in shaping perceptions and assimilations of foreign segments. The goal of this chapter is to verify with representative data from Miami Spanish some following types of assumptions about Natural Loanword Phonology.

Hypothesis I: A language's ability to borrow words is to a major extent determined by what shapes it permits at the surface level; that is to say, it is generally the late syntagmatic processes and not the underlying restrictions (paradigmatic processes or morphological rules) which are relevant in determining what can be borrowed and which operate on what lexical items eventually are borrowed into a target language.

Hypothesis II: Loanwords have underlying representations first established at a depth corresponding to Stampe's notion of the natural phonemic level as a level of linguistic reality.

Hypothesis III: Only processes, which are "alive" and productive in the grammar, and not rules, which remain "dead" and unproductive, will function in the initial establishing of a loanword in the target language.

Hypothesis IV: Lexical forms of the target language are definable as either native forms (those to which all "rules" have applied wherever applicable); assimilated loans (those to which all available processes have applied); and unassimilated loans (those which contain violations of processes constraining the underlying representation and are therefore "phonological" exceptions, and those which contain violations of processes determining surface representation and are therefore "phonetic"

exceptions).

Hypothesis V: Substitution processes which will not serve to predict structures of elements entering the target language are easily classifiable as rules rather than processes, and this is, in fact, one foolproof measure of what is a "rule" and what is a "process" for any natural language.

These assumptions are tested with the treatment of abundant English loanwords adopted by Miami Cuban speakers of Spanish. One conclusion which seems incontrovertible in light of data available is that moribund "rules" are clearly irrelevant to initial lexicalization of loanwords in the target language, though they do likely afterwards apply at some post-borrowing stage to bring about greater "nativization" of foreign segments.

In Part Four a developing theory of Syllabic Phonology is discussed in relation to both the proposals of Natural Phonology and the demands of loanword phonology. This theory is one based on the postulation of "weakening chains" in Natural Phonology (Nessly 1973) and modified along lines suggested by Hooper (1973) relative to positive syllable structure conditions for Spanish, which in turn build upon a notion of universal consonantal strength hierarchies. The object of study here is speakers' apparent strategies for syllable structure modification if borrowing takes place from some language with complex syllable structure (English) to one with less complex syllable structure (Spanish). Attempts are made to demonstrate quite irrefutably that the modifications of syllable structure in at least this one dialect are more insightfully explained when assumptions of the Stampean natural phonologist are adopted as a standard. Heavily syllable-based schemes such as Hooper's version of

Natural Generative Phonology are, on the other hand, neither explanatory nor even consistently accurate in their predictions.

In brief, this dissertation establishes why loanword phonology is central to progress in phonological theory in general. This primacy of loanword phonology stems, of course, from the fact that the structure of a lexicon is product of the workings of a phonological system whose functions and whose mechanisms are nowhere better illustrated and more available for study than in the processes of linguistic borrowing.

This present study is motivated throughout, then, by an assumption that Natural Phonology can best approach precisely those problems raised by linguistic borrowing which have remained insoluable within several previous frameworks for grammatical study. One reason seemingly is the explicit distinctions set forth in this theory between processes and acquired rules. Another is the focus on casual speech phenomena. A third is the positing in Natural Phonology of quite non-abstract phonetically and mentalistically motivated underlying representations for all surface strings. But the primary advantage of Natural Phonology over generative phonology or structuralist phonology or stratificational phonology emerges from the fact that the latter undisputably are all descriptive enterprises while only the former provides us with a truly explanatory model of linguistic behavior.

1.2 Claims of Natural Phonology

Stampe's hypotheses governing the acquisition and development of phonological systems may be briefly and somewhat superficially outlined for this chapter after the following fashion, occasionally paraphrasing here from Stampe's own discussions in Stampe 1969 and elsewhere.

1.2.1 The innateness of phonological processes

A phonological system for any speaker of any language is the residue of an innate system of phonological substitution processes, already possessed in its entirety by the infant first encountering his language and subsequently revised extensively during the course of the child's early acquisition of his native speech.[3] Stampe defines a phonological "process" as

> a mental operation that applies in speech to substitute, for a class of sounds or sound sequences presenting a specific common difficulty to the speech capacity of the individual, an alternative class identical but lacking the difficult property (1973a, page 1).

This is a way of saying that processes merge potential oppositions into members which present the least obstacle to the speech-producing mechanisms. To illustrate: in an environment favoring voicing (such as intervocalic position) such a merger would voice all obstruents, which by nature and irrespective of context are, in the absence of such processes, more easily articulated as voiceless. The pressure to devoice obstruents (i.e. to produce them as voiceless) irrespective of context -- their oral constriction being an impediment to an airflow needed for voicing -- but yet to voice them between vowels demonstrates that processes by nature come in contradictory sets revealing conflicting phonetic restrictions. When such conflicting tendencies arise, Stampe assumes the speaker will resolve them by means of three observable mechanisms: (1) he suppresses one of the set of contradictory processes; or (2) he places some limitation on the set of segments to which a process will apply or the context in which it applies, and thus he suppresses some part of the process; or (3) he orders the application of the processes to achieve a particular output. From

babbling to the achievement of adult pronunciations, the suppression, limitation, and ordering of processes results in the considerable revision of the child's innate system of phonological substitutions.

1.2.2 The restrictive nature of phonological processes

The inherited innate phonological system represents a complete "potential" system of phonological as well as mental restrictions on speech, and these are manifested in a type of process which makes real substitutions, even though this type clearly differs ontologically and teleologically from the learned "rules" in the phonology. Stampean processes are restrictions on phonological output, but they are not static conditions in the sense of the templates which are strictures on morpheme structure (Halle 1973) or in the sense of redundancy rules as these are conceived in the standard theory (Stanley 1967).[4]

We must speak here of a "potential" system, for despite a reasonable similarity in approach, different children learning the same language will display different sets of processes, and in a variety of possible ordering (Stampe 1973a; Ingram 1974; Salus and Salus 1974). Also, the inventory of processes relevant to a single language (or a single speaker of a single language) is only a portion of the total inventory available to each new human speaker.

With the onset of the child's speech in the babbling stage the natural processes appear to be entirely unordered, and the most extreme processes are, in fact, directly observable only at this primitive stage (Stampe 1969, page 444). The earliest substitutions of the child's phonology are only these more extreme processes: e.g. unstressed (weak) syllable deletion, cluster reduction, obstruents becoming lax stops, merger of vowels to single central articulations

(the maximally vocalic a) (Stampe 1969; Salus and Salus 1974). From the outset the child's speech will display both syntagmatic processes, which function to ease transitions from one segment to another, and paradigmatic processes, which establish properties of individual segments, though the preponderance of earliest processes is in the direction of the paradigmatic. Only with young children do the paradigmatic processes typically apply in "feeding" orders, and that this should be so is a natural outcome of the fact that adult speech would find intolerable the massive kinds of neutralizations of underlying distinctions which mark infant speech and which several paradigmatic processes applying in a feeding relationship would work to produce (Donegan Miller 1973b).

1.2.3 The revisions of the innate system

The acquisition of new phonetic oppositions (i.e. new single segments) by the child entails a constant revision of innate processes of his phonological system. We have already noted Stampe's central claim that the mechanisms for resolving conflicting processes are the same mechanisms relied upon for revising the innate system. By his suppression, limitation, and ordering, the child continually revises all apsects of his system which distinguish his pronunciations from the standards of adults around him -- until his system in fact becomes the adult system. That massive variability may occur in children's substitutions, or that the child may persist as long as he does in apparently perceiving and reproducing his own unique system, are both only apparent anomalies which offer no real contradiction of the theory proposed. Each case can be readily enough explained: in the first instance there is persistent correction of children's pronunciations

by surrounding adults, and in the second there is the lack of an opportunity for the child to have conversation which would provide the necessary feedback telling him whether or not his imitations have been in any measure successful (Stampe 1973a).

1.2.4 The natural order of acquisition

Notable regularities can be observed (see Stampe 1973a; Salus and Salus 1974; Ingram 1974) in the order in which children acquire and master phonetic segments, and it is a proposition of Natural Phonology that this natural order of acquisition is again readily explained by the independently attested innate system. That is, the workings of language acquisition will be fully accounted for by the hierarchical arrangement and interactions of the natural processes.

A serious implication for child language theory as well as for general phonological theory is the observation that Stampe's system renders unnecessary Jakobson's hierarchical structural laws of irreversible solidarity (cf. Jakobson 1968; Salus and Salus 1974; and Stoel 1974). Stampe argues (in Stampe 1973c) that, like Chomsky and Halle's "markedness" conventions, Jakobson's assumed implicational laws which purportedly account for the system of language acquisition (in children) and phenomena of language dissolution (in aphasics) are mere appearances resulting from what is in actuality the underlying system of innate natural substitution processes. Evidence for the reality of active processes opposed to the possible reality of implicational laws or conjectured reality of marking conventions hopefully will become more apparent as notions of Natural Phonology are more ambitiously expanded throughout sections which make up Chapter Three below.

1.2.5 The internal representations of adult speech

Although it is a fair assumption that the child passes through an early stage of initial phonological development marked by incorrect perceptions of an adult sound system (cf. Golick 1974), it is equally apparent that mental representations which make up his own underlying system are very closely based on the adult models (Stampe 1969, Ingram 1974). Ingram (1974) expresses a view in line with Natural Phonology when he suggests that "the mental representation (by the child) of the adult model is not just the result of inadequate perception, but also the result of organizational principles that the child uses to systematize this data" (pages 51-52).

An even stronger claim of Natural Phonology is that the child will internalize representations of adult speech which are far more sophisticated than his own reduced reproductions would indicate. Stampe has cited as one example his own son's rendering of "dog" as first [da] (the same as for "doll" with no evidence of any final velar), then [ga] (with the initial coronal assimilated to the deleted final velar), and finally [gag] (with this final velar now pronounceable). Also bearing on the issue is Jakobson's example, borrowed from Nadoleczny, of the child who says in German first Duten Ta Herr Dotta, and then Guken Gag Herr Goka, for "Guten Tag, Herr Doktor" (Stampe 1969, pages 446-47), another case which suggests strongly that children mentally obtain phonological representations considerably before they can begin to utter them.

Natural Phonology, then, has taken an extremely strong position in the current controversy (cf. Kornfeld 1971) over whether the mature adult system of phonological distinctions determines the child's system

or whether the child perceives as well as produces his own entirely unique system of phonology. Stampe maintains the child's representations are in terms of the adult's surface "phonemic" forms, which are inputs to the system of natural substitution processes.[5] This assumption is a main subject of the discussion within Chapter Three which deals, among other things, with Stampean phonemics versus classical phonemics. For the present suffice it to say that the adult perceived-phonetic forms (perceived, that is, by the child) plus the system of natural processes (and eventually some "learned" rules as well) result in the child's phonetic outputs. This particular interpretation can be and has been expanded by Stampe into a Phonemic Hypothesis (cf. Section 3.1.2), which claims that child and adult speakers alike deal most frequently with shallow phonemic patterns much less abstract than the lexical representations envisioned to have psychological reality in Chomsly and Halle's Sound Pattern of English (simply SPE throughout the remainder of the text).

1.2.6 The innate residue in adult grammars

The fully mature adult system of speech can be assumed, then, under this framework, to retain all aspects of the innate system which this mastering of pronunciation has left intact. Such a system maintains as well the learned set of substitutions which are the phonological rules and morphological rules also governing pronunciations -- though not governing pronounceability. It is those innate processes which fortuitously survive the course of language acquisition which actually determine what are pronounceable (or at least what speakers assume are the actually pronounceable) phonetic representations of the language. For English speakers, the aspiration of initial voiceless stops is one

such surviving process, while the rampant deletion of final velars or the devoicing of all final obstruents assuredly are not (though they might be observed for a period of time in the pronunciations of many of the children learning to speak English).

This last-mentioned example of obstruent devoicing is especially suggestive and much has already been made of it in the literature on Natural Phonology (esp. in Stampe 1968, 1969; Kiparsky 1968b). Thus a highly-motivated process devoicing word-final obstruents appears in many languages of the world almost simultaneously with the first acquisitions of this type of segment. To master pronouncing the voiced as well as the voiceless obstruents, then, the English-speaking child must learn to suppress the process; but German-speaking children, faced with a language in which such a process remains active, need not. In fact, they must not suppress devoicing if they are to speak acceptable German. In German this devoicing process turns out to govern purely phonetic but not phonological environments, as the always cited alternation of Bunt "association" with its plural Bunde "associations" and dozens of similar forms reveal. With some few languages (e.g. Hawaiian) it governs phonological representations as well and there are no voicing oppositions even on the most highly abstract levels.

In this last case Stampe goes still further to contend that in languages without any final stops (or even any final consonants at all), such a process still remains part of the speaker's potential inventory, a claim which he forcibly admits is unacceptable to any other current system of phonology. But this unorthodox thesis felicitously turns out to be enhanced by one small but very impressive

piece of evidence: speakers of these languages will regularly devoice final obstruents once they are forced to confront these for the first time in foreign loanwords, despite the total absence of any parallel cases in their own dialects (Stampe 1969, page 445).

1.2.7 Naturalness motivations in linguistic change

In preceding paragraphs I have suggested something of the force of Stampe's proposals in pointing towards more explanatory models for synchronic grammars. More controversial proposals in light of the traditional models are those which involve linguistic change, on the one hand, and the phonologically central role of borrowing phenomena, on another.

A normative view of linguistic change is that rules may be added, lost, generalized, or complicated in a grammar (cf. Miller 1973; King 1973, 1974; Dinnsen 1974). But one troublesome issue throughout has been why rules should be added or complicated in the first place in a process of linguistic change which is apparently motivated by the pressures in languages toward overall simplification (Miller 1973). In the theory of Natural Phonology advanced by Stampe, the system of innate processes which accounts for language acquisition again serves to explain as well these related events of phonetic change.

For Stampe, phonetic change results only when "the child fails to suppress some innate process which does not apply in the standard language" (1969, page 448). More generally, rule addition, generalization, and unordering of processes are direct manifestations of the child's failure to suppress (rule addition), to limit (rule generalization), or to order (rule unordering) natural processes with just those three mechanisms mentioned for the revision of innate systems.

Such phonetic change is limited to the degree that the conservatism of all speech communities assures the rejection of most innovations by children. And when innovations in child speech are admitted in the adult speech patterns, they usually are so only most gradually, i.e. only as optional pronunciations.

There are additional reasons for opting for Stampean diachronic linguistics. A view of phonetic change such as Stampe proposes has the one special feature of accounting for exactly why change does not seem to occur with adult speakers (except in some rare cases through the influences of linguistic borrowing), why it is at times quite radical, but why at the same time it is always astoundingly regular. For, "if a child fails to master a certain sound, it will appear that he has changed it to the sound he regularly substituted for it" (Stampe 1969, page 448). Notice that this view of historical change refers only to natural phonological processes and makes no references whatsoever to learned and unproductive phonological "rules". This is a feature of another current version of natural phonology as well -- Vennemann's Natural Generative Phonology. All new rules are phonetically motivated and clearly have their source in some universal inventory of phonetic constraints or conditions. Rules are simply not added to grammars when they have grammatical conditioning or when they reveal surface exceptions. Both Stampe and Vennemann draw a similar (though not identical) distinction between the natural phonological processes (the "phonetic rules" for Vennemann) and acquired phonological rules (the "morpho-syntactic rules" for Vennemann) within the amorphous class of substitution processes lumped together in a standard generative theory as the "phonological rules" (see Chapter

Eight for fuller discussion of Vennemann versus Stampe).

Stampe's view that phonetic change follows only from the child's failure to suppress some natural process which should not apply in the standard language, and therefore that it does not involve learned rules at all, is to be sharply contrasted with a view of Jakobson, Halle, Kiparsky, and others, that change is also motivated by addition and at times suppression of more cognitive phonological processes. I explore this distinction further in Sections 3.1.1 and 3.1.3 within Chapter Three and give there three particularly illustrative examples based on current literature (Daniels 1973; Cressey 1974; Dinnsen 1974) of how phonetic change might be more insightfully interpreted in the framework of a Natural Phonology than in the framework of a standard generative phonology. The position of Stampe versus that of Halle or Kiparsky should, of course, be open to an empirical verification and such verifications will also be taken up in Chapter Two and Chapter Three (as will the empirical support for Stampe's model over that of Vennemann). In essence, Stampe contends that the few recorded genuine cases of the suppression of natural processes (e.g. loss of final obstruent devoicing from Yiddish and Swiss dialects of German) are exceedingly rare and can at any rate be sufficiently explained away by a complex of factors that seem to have nothing at all to do with the pressures toward grammatical simplification (see note twelve for Chapter Three). The result is clearly that suppression may be listed among the possible but not among the primary mechanisms of language change.

1.2.8 The prominence of natural loanword phonology

Stampe 1969 asserts byt makes no effort to demonstrate that "the

problem of 'phonological admissibility' is contained in, and therefore inseparable from, the larger problem of loan phonology" (page 451, note 4). To a degree at least this present study will attempt just such a demonstration, offering one of the more extensive although certainly not the first investigation along these lines.

Earlier though woefully incomplete attempts to establish the intimate connections between principles of Natural Phonology and the events of loanword phonology are represented by the work of Mieko Ohso (1971) and Julie Lovins (1973, 1974a, 1974b). Especially in Lovins 1974b conclusions are drawn from Japanese lexical study which would seem to support the contention that Stampe's view of an innate system of processes explains events of linguistic borrowing as readily as it accounts satisfactorily for events of infant speech or processes of linguistic change. Since they motivate as well as sustain my own observations about borrowings, Lovins's conclusions are valuable to quote here in some detail, precisely as she has arranged them throughout Lovins 1974b. These proceed as follows:

> (1) Perception of the "closest sound" in another language occurs in terms of phonological processes, not binary features: features are not perceptual primes (page 242).
>
> (2) Systematic non-uniform criteriality of features, dependent on the phonetic substance of segments, corresponds to this large inventory of processes. This also explains the existence (though not the distribution) of alternative substitutions, since more than one process may apply to a given segment (page 242).
>
> (3) Processes governing the phonological system of a language sometimes appear explicitly only in an interference situation. Often those processes constraining underlying representation will determine the "closest sound" (page 242).
>
> (4) Allophonic distribution must be considered in characterizing the "closest sound".....Sounds are often perceived sequentially, not individually, and in relation to context-sensitive processes[6] (page 243).

(5) A context-free process determines perception of a foreign sound only if no relevant context-sensitive process is available to do so[7] (page 243).

(6) Loanwords have underlying representation at the phonemic level .....Since morphophonemic processes determine underlying representations only when this is required by specific morpheme alternations, and loanwords are not involved in such alternations, they are automatically represented phonemically. This fact has sometimes been remarked on as one aspect of the "exceptionality" of loanwords, but this type of "exception" is merely a demonstration of the over-abstract underlying representations being forced upon non-foreign morphemes in Language T by linguists (where "T" is "target" language -- PCB)[8] (page 243).

(7) When we speak, we apply allophonic processes "forwards" to produce contextual variants; when we listen to someone else, we apply them "backwards" to relate the allophones to their associated phonemes, automatically considering a sequence of as many segments at a time as are involved (as context or derivates) in a derivation. Likewise, in listening to unfamiliar foreign sounds, we try to relate what we hear to possible surface forms in our language. These surface forms may already be acceptable underlying representations, or related to such by backwards-derivation of an allophonic process..... In the "neither" case, already mentioned, one has two options: apply a context-free process governing underlying representations, to get an acceptable one; or backwards-derive the segment up to the point where it is a phonological exception before doing this (i.e. before applying the context-free process -- PCB) (page 244).

(8) The "environment" part of a context-sensitive process may be generalized in the perception of a foreign sound sequence..... Stampe has pointed out that this is what happens when we listen to "baby talk": a yound child's phonology is different from ours largely in that a lot more processes apply, or they apply more generally; in order to understand what the child is saying, we have to relate what is normally the output of our derivations to his less restricted phonetic forms. To the extent that we can still do this we can correctly interpret what he is saying (page 244).

(9) When a foreign segment appears in an environment in which the equivalent native derived segment does not appear, then the form of the incoming foreign word is modified so that the structural description of that rule is met and the segment in question is then derived in the appropriate environment (page 246).

(10) When a sequence of foreign sounds not corresponding to a legal derived sequence in the target language is interpreted according to a context-sensitive process, the environment of this process will be generalized to the smallest natural class that encompasses the corresponding segment in the source sequence (n.b. this is a less formal restatement of the previous theorem -- PCB) (page 246).

(11) The "degree of nativization" of a loanword is determined by what processes have applied in its lexicalization, if there is a choice; and what rules, if any, have applied after its initial lexicalization (leading to relexicalization) (page 248).

(12) "Native" forms are those to which all (or most?) rules have applied, as applicable. "Assimilated" loans are those to which all processes have applied. "Non-assimilated" loans contain either a violation of a process constraining underlying representation (are phonological exceptions) or of one that normally determines surface representation (are phonetic exceptions) (page 248).

As it is codified and summarized in these dozen theorems, then, Lovins's work takes an ambitious initial step toward elucidating some conditions under which each of two important processes responsible for lexicalization in Natural Phonology apply in the course of lexical borrowing. These are the forwards application of the higher-level morphophonemic processes and the backwards application of lower-level allophonic ones. Tentatively at least, Lovins provides the first formulation of a significant principle: that "at almost all points the elusive concept of 'closest segment' in two languages is clarified in terms of these two conditions" (1973, page 2). A more general but nonetheless equally significant outgrowth of Lovins's study of foreign borrowings would also have to be the observation that innate processes as Stampe has conceived of them maintain the most vital bearing on loanword phonology, with the role of "rules" in borrowing still being largely indeterminant though most likely not overly productive.

It seems reasonable, then, that Lovins should assume that looking for an adequate theory to account for the behavior of loanwords in essence is equivalent to "arguing for the efficacy of a theory of natural phonology such as proposed by Stampe.....and arguing against some tenets of nonrevisionist generative phonology" (1974b, page 240). However, a weakness of Lovins's work (as for that matter of Stampe's)

is the absence of any widespread empirical verifications, being based as this research is on the necessarily limiting data from a single source language. Natural Loanword Phonology (Natural Phonology in general) remains, for most linguists familiar with it at all, an untested theory which raises considerable scepticisms and even numerous hostilities. Part of the reason no doubt has been advanced by two recent commentaries which suggest that Stampe's collective theory is not only untested but perhaps virtually untestable as well (see Stoel 1974 and Ohala 1974).[9] This has from the first been an altogether familiar and unfortunate categorization of Stampe (n.b., however, Ohala's retraction quoted below in note 9). It is as rebuttal to particularly this estimation of Stampe (the untestability hypothesis) that I have pressed my own conviction here: i.e. that it is not only possible but even eminently reasonable to assume that such hypotheses about loanword phonology as those formulated by Lovins and those derivable directly from Stampe (1973a) are not only susceptable to testing but even highly compatable with empirical verification or disconfirmation -- and along with them the general tenets of Natural Phonology as well. While much of this dissertation (Part One and Part Two) is of necessity devoted to merely explaining Natural Phonology, in concluding chapters I have exposed Lovins's hypotheses, along with others which derive directly from them, to extensive additional analysis in light of renderings of numerous English loanwords in the Miami Cuban dialect of Spanish. One welcome result of Chapter Seven and Chapter Eight, for example, is the expected conclusion that loanword phonology is not only considerably clarified by application of Stampe's theories but also itself supplies in return what seem to be the necessary verifications

for Natural Phonology, proofs which are otherwise not always apparent from related areas of exploration in natural languages -- such as child phonology, speech errors, or observable evidence from the adult system of phonology and especially from casual speech phenomena.

To review briefly, what I have given in this section is a summary of the foundational claims about Natural Phonology. I have been most concerned here with emphasizing (1) the innate system of phonological processes; (2) the physiological and mentalistic motivation of these processes versus the abstract and highly unproductive status of distinct "rules"; and (3) some implications, at least, of Stampe's notion of processes for possible interpretations of phonetic change and of a synchronic loanword phonology. But what is presented here is at best a superficial outline of only some of these hypotheses fundamental to Stampe's theory. Throughout the first half of this present work these claims are more thoroughly explored and, to whatever extent possible, empirically motivated. Throughout these earlier chapters, I will attempt to view Natural Phonology primarily from the standpoint of the reality and productivity of the innate processes, both as they operate in historical change through time and as they function in the acquisition of individual grammars. Eventually, of course, the vested interest of this dissertation will be in the specific issue of loanword phonology and in particular cases of contact borrowing within a single synchronic grammar, and these will be taken up in detail with Chapter Five, Chapter Seven, and Chapter Eight.

1.3 Explanation and Performance Models

At the forefront of Stampe's work on Natural Phonology is the

assumption that our recognition of an innate system of processes will make available to us explanations for linguistic behavior not possible with the standard generative phonological theories. At least not as these theories, which might be referred to collectively as "abstract generative phonology" (Terrell 1975d), have been previously conceived. The subject matter of the few existing monographs and articles on Natural Phonology produced to date has persistently involved some attempts at just these types of explanation, like those we have already hinted at for language change and linguistic borrowing, for example.

Another topic prevalent in the writings on Natural Phonology (esp. in Stampe 1969, 1973a) has been the considerable evidence that innate physiological and mental processes are, in point of fact, true substitutions actively applying in psychologically real grammars. Yet to date the bulk of these discussions focus on evidence from child language research which, as we have just noted, is only one (though perhaps the most exploitable one) among several obvious testing grounds. The work which we have earlier alluded to (Section 1.2.8) by Lovins seems sufficient to suggest that the observable loanword material is equally well adaptable to this framework and therefore equally supportive of Stampe's principles. But we have just finished claiming that Lovins's studies in another sense only serve to emphasize for us the huge gaps surrounding existing studies on loanword phonology, and it is with the hope of filling some of these gaps and defining some of the intractable issues that my own investigations of phonetic borrowing in Cuban Spanish have been undertaken.

It is to be borne in mind then, throughout this and other writings devoted to Natural Phonology, that Stampe's version of "naturalness"

takes the goal of a fully adequate linguistic theory to be one of "explanation" in phonology. This is a marked departure from the early goal of Chomsky, which is unambiguously stated to be no more than "the construction of a grammar" (Chomsky and Halle 1968, page 3), where a grammar of a language "purports to be a description of the ideal speaker-hearer's intrinsic competence" (Chomsky 1965, page 4).

To establish Stampe's contrasting commitment to "explanation" as the true and single goal of phonology, it is necessary only to point to some recent assessments he has made of Vennemann. One unpublished observation, for example, rejects quite out of hand Vennemann's label of "natural generative phonology" because it misses precisely the point of a distinction between Natural Phonology and generative phonology: that "even at the level of universal grammar generative phonology is a descriptive rather than an explanatory enterprise" (Stampe personal communication). Stampe takes "generate," then, i.e. in the sense employed by Chomsky, to mean more narrowly "to describe all and only" (Stampe personal communication). The broader goal of any linguistic theory for the natural phonologist is rather "to understand language, to see how its peculiarities follow necessarily from the nature of its function, its implementation, and its use" (Stampe personal communication).

In light of Stampe's own careful dichotomy between "natural" and "generative" as distinct and even irreconcilable approaches, it would seem essential that we emphasize foremost here Natural Phonology (as so far proposed by Stampe, by Rhodes and Patricia Donegan Miller, and in my own papers in Bjarkman 1974a, 1975) as a radical departure from standard generative linguistics. In several important senses Stampe's

model is clearly a completely different theory from Chomsky's, rather than perhaps a close notational variant of those types of phonological analysis envisioned in The Sound Pattern of English or adopted by Postal, McCawley, Kiparsky, or most other orthodox transformationalists. (One overview of this uniqueness of Natural Phonology is now to be found in Bailey 1975.) For one thing, I will argue extensively in Chapter Three that Stampe's concept of natural processes is not at all reducible to any SPE notion of the late phonetic rules. This is largely of course because Natural Phonology, as already underscored, pays little attention to any categories among the "acquired" rules, other than in the negative sense of assuming that most rules proposed in SPE for English have the status of processes and are not "rules" at all in the Chomskyan sense of the term.[10] That is, the division between rules and processes occurs (or at least is envisioned by the linguist) deeper in the grammar than a systematic phonetic level; or put otherwise, processes are not simply detail rules with n-ary feature scales but have in many cases true morphophonemic as well as allophonic significance for the grammar.

Yet the real departure of Stampe's version of Natural Phonology is that within a general framework which can still be considered loosely to be generative grammar it makes giant strides toward achieving a true performance model of phonology.

Generative grammarians collectively have remained largely uncertain about the role of "performance" in the idealized formal grammars they write. Therefore one recent study, by Derwing (1973), is for just this reason a considerable achievement, if only in the sense that it brings about some lucidity in Chomsky's otherwise confusing and shifting stance on what would constitute a competence/performance distinction in the

interpretation of generative grammars. Derwing unravels Chomsky's final position to be essentially uncompromising in his opposition to applying strict performance criteria:

> Transformational-generative grammars are not intended to be interpreted as idealized production and/or perception models, after all [e.g. after the fashion of Saussure's famous metaphor that a language is comparable to a symphony and speech to the performance of that symphony -- PCB], but as models of something else "neutral" between the two; in other words, a model of "linguistic competence" (as this "something else" has since come to be known) is not to be regarded as an idealized model of linguistic performance (Derwing 1973, page 265).

In other terms, while Chomsky repeatedly speaks of the desirability of models to test linguistic performance, he strenuously avoids the close interpretation of generative grammar in this vein. And Chomsky himself again echoes this point with his restatement in Aspects of the Theory of Syntax:

> To avoid what has been a continuing misunderstanding, it is perhaps worth while to reiterate that a generative grammar is not a model for a speaker or a hearer. It attempts to characterize in the most neutral terms possible the knowledge of the language that provides a basis for actual use of language by a speaker-hearer.....When we say that a sentence has a certain derivation with respect to a particular generative grammar, we say nothing about how the speaker or hearer might proceed in some practical or efficient way to construct such a derivation. These questions belong to the theory of language use -- the theory of performance (1965, page 9).

In light of such passages it is all the more significant that Stampe has begun his own discussions of the organization of a system of natural processes with the observation that "there has been a tendency in linguistics to view processes of grammar as descriptions of the language 'competence' of speakers, and not the actual processes that occur in the production or perception ('performance') of speech" (1973a, page 43). Thus inevitably, "explanations of linguistic phenomena based on 'as-if' descriptions remain 'as-if' explanations" and, what is perhaps worse,

miss the vital generalization that "the conditions of the use of language (performance) are responsible for the nature of language" (1973a, page 43, with Stampe's own underlying). As again Derwing (1973) emphasizes with a special urgency, it is this absence of a performance grammar which is the peculiarly debilitating feature of the SPE notion of the phonology.[11] McCawley notes of Derwing (and thus indirectly about the transformationalists that "he is particularly critical of the failure of transformational grammarians to concern themselves with performance to any greater extent than merely formulating programs for its investigation" (McCawley 1974, page 178). Natural Phonology is by stark contrast a genuine performance model, if only to the degree that it takes the psychological reality of substitution processes rather than economy criteria and simplicity measures as the proper constraint on highly-valued grammars. The natural phonologist does not exactly advocate that linguists set out to write grammars which are uneconomical, though he would presumably advocate that they more consistently use the data from linguistic change and from psychological experiments to try to uncover exactly what grammars are really like (Darden 1971, page 330).

A second acknowledged and not altogether unrelated weakness of generative phonology as it stands has been the absence in an SPE model of any true theory of lexical representation. Once again it is Derwing (1973, pages 127-28) especially who has captured the subtle implications of such oversights:

> A related, and equally disturbing, development in current phonological theory is that underlying representations for morphemes appear to be getting more and more like the conventional orthographic ones.....But if this is so, we are faced with a strange anomaly: why do the problems encountered by English-

> speaking children in learning their own "near optimal" spelling system (in which the orthographic representation of morphemes corresponds very closely to their abstract lexical representations in a generative grammar) far outweigh the minimal difficulties which the typical Russian child appears to have in learning to spell Russian (where the orthography diverges greatly from Lightner's level of lexical representation, and parallels instead the less abstract morphophonemic representation of the sort proposed by Jakobson).

The causes are of course obtrusive at this point. Yet part of the reason at least would seem to follow from the cogent observation of another recent critic of nonrevisionist generative grammar, Mary Louise Clayton, who contends that a major flaw in the theory is that "there is no clear principle establishing the degree to which a phonological representation can legitimately differ from its surface representation(s) on the one hand and its lexical representation on the other" (Clayton 1974, page 155).

Clayton would seem to relate this anomaly to the fact that "an assumption apparently arose generally in the field that only phonological rules were necessary to relate surface representations to underlying (lexical) representations" (1974, page 160). The flaw in such an assumption is clearly that it "confused the issues of the abstractness of phonological representations, which is a question of the nature of phonological rules, and the abstractness of lexical representations, which is a question of what alternations should be accounted for by the grammar" (page 160).

Clayton concludes her assessment of the issue on a note of surprise that, although Kiparsky (1968a) takes up the "abstractness" question, a notion of "strictly phonological" as a desirable restriction on what should be admitted as "phonological rules" (the topic of her own study) has not surfaced until works of a much more recent vintage. But of

course none of this would be particularly surprising is we recall that until very recent and unprecedented work by, for example, Halle (1973) and Skousen (1973, 1974) there has been no approach which allows for "morphological" rules which are in any substantive way distinct from the amorphous phonological component which considers all "phonological" substitutions to be quite identical and inseparable by nature (compare e.g. Schane 1971).

The failure to provide a convincing theory of lexical representation within generative grammar should in itself serve as a sufficient motive for the goals and organization of this present study. Whereas in the first third of this thesis I undertake describing Natural Phonology in terms of a performance model of phonological grammar, with Chapter Four I also make some tentative first proposals about what a lexicon must look like for an adequate natural generative grammar. These are proposals which combine the standard notions of grammatical components (syntactic, morphological, lexical, phonological, and semantic) and lexical insertion with Stampe's views of what comprises a natural system of phonological rules. My model is discovered to be much like that found in Halle 1973 or implicit in Harris 1974. It assumes that there is for each speaker a dictionary of lexical items which results from applications of word formation rules and an exception filter on lists of formatives (morphemes) and that it is within this separate notion of Morphological Component (i.e. before the fact of lexical insertion) that many of the alternations previously taken for "phonological" rules will actually apply.

A superficially similar proposal for investing generative grammars with independent morphological rules is already to be found in the

miscellaneous work of Royal Skousen (1972, 1973, 1974).[12] For Skousen, however, a phonology is made up of rules which account for two distinct kinds of output regularities: (a) fully productive rules which are phonetically motivated and always entirely exceptionless in their applications; and (b) rules of "morphological alternations" which are replacements in the synchronic grammar for rules that at one time were (but now no longer are) phonetic and exceptionless. (Skousen's rather impressive evidence that speakers do not always capture underlying regularities that are products of the historical rules turns out to be a case where speakers of Finnish change new words to respond to surface patterns of Finnish by corresponding with other forms undergoing the historical rules, even though this means directly contradicting some phonetically-plausible rules.) One immediate problem for Skousen's analysis (when opposed to that of standard theory or that of Halle's ideas of word formation) is that it makes no apparent provision for the vast majority of rules which are neither phonetically-plausible nor attested by morphological alternation and which comprise the largest class of rules generally called phonological rules by SPE. In Skousen 1973 this difficulty is seemingly resolved in a most curious way. The solution is to have morphophonemic alternations (those relating what generative phonologists call the systematic phonemic and systematic phonetic representations) accounted for by morphological rules which may well be phonetically-conditioned but do not stand as phonetically-plausible generalizations about the grammar. This third category of rule comes about when a number of morphological rules state over and again an identical alternation in the identical phonological environments. Skousen proposes that speakers, then, are able to recapture historical

environments for "dead" rules like e.g. Gemination in Finnish precisely because "the process of postulating morphological rules leads directly to the original rules.....the evidence for the phonetically statable environment is discovered by an acquisition procedure that depends upon morphologically-defined surface rules" (1973, page 25). It remains undetermined with Skousen's theory what kind of a "level" is relevant for stating morphological rules, though it is suggested that "a theory using morphological rules implies, in fact, that underlying representations are not very deep at all" (page 22) and also that "analogical change suggests that the linguistically significant level in phonology is the surface phonemic level" (page 22).

In Skousen's papers we uncover a phonological system that sounds vaguely akin to that outlined for Stampean phonology (see Section 1.2) earlier within this chapter: a traditional phonological component is reassessed as comprising learned non-productive generalizations (for Skousen morphological alternations) and exceptionless phonetically plausible rules, and a linguistically relevant surface phonemic level. Skousen's morphological rules are, from what he reveals about them, what taxonomic phonemics would call morphophonemic processes, while his phonetic rules approximate the allophonic processes of the structuralists. Such a format allows distinctions (actually it claims that speakers make such distinctions) not available in an SPE notion of phonology, where speakers are assumed to have one underlying phonological level (the systematic phonemic or lexical level) and where it is also assumed that a single type of rule will derive from this level both the morphophonemic and allophonic alternations without any provision for distinguishing which are of

which type.

But like other recent arguments for restricting phonological rules to productive phonetic rules (see Clayton 1974), Skousen's model is only vaguely defined and therefore not readily testable. Most of what follows is devoted to arguing advantages of a modified version of the Stampean proposals over a partial and largely unempirical system like Skousen's. What is most problematical about the latter is that if Skousen recognizes distinct types among phonological alternations, he never indicates how actual speakers might ever do this, nor how deep the "not very deep" underlying representations are and by what principles this could be determined. An insurmountable difficulty for Skousen (though not at all one for Stampe) seems to be also that the morphophonemic rules are sometimes actually phonetic in motivation (see my discussion of German final devoicing processes in Section 2.3). The notion of morphology suggested here in Chapter Four, on the other hand, is one in which the morphological rules are word formation conditions within a distinct but "passive" morphological component (i.e. the lexicon) and thus remain outside altogether the province of active derivations containing morphophonemic or allophonic processes. Morphological rules constitute a truly "passive" grammar -- in the sense that the speaker only applies them when needed to account for some new or unfamiliar form. It is this idea of morphology as a separate component of the grammar which makes possible explanations for rather intractable problems in earlier approaches to loanword phonology; as a case in point, it is surprisingly easy to demonstrate that Cearly (1974) is irrecoverably wrong when he asserts that phonological rules apply indiscriminately to foreign forms but (what he

calls) morphological rules never manifest themselves in this way (see Section 7.3 below). The idea of a "passive" grammar also proves to be advantageous in accounting for how such rules can seemingly be inserted at random points throughout a derivation, in apparent contradiction of the generally accepted ordering principle of first the morphological, then phonological, and finally phonetic rules. The sustaining of such an ordering principle is a difficulty earlier noticed (especially in Skousen 1972) and both Skousen and Anderson (1974, 1975) provide ample illustration of phonetically conditioned rules followed by morphological rules (e.g. in English the rule governing a/an distribution applies to outputs of initial h deletion), a case which proves embarrassing to any notion of linearly-ordered strings of morpholexical, phonological, and phonetic rules. My own conception of a phonological component (Section 4.2.0) would seem to successfully avoid such perplexities in rule ordering.

I have given some attention to Skousen's approach to morphophonology here since it appears to represent a single serious attempt, outside of those of Halle and Harris, to formalize a morphological component within a generative grammar. Further studies in this vein are exceedingly rare and often only obliquely related to the topic at hand.

Saciuk's dissertation (Saciuk 1969), for instance, provides an exceptionally ambitious effort to study "the role that the components of the lexicon play in generative phonology, and to sketch a proposal for dealing with these phenomena" (1969, page iv). Like several known predecessors (and he cites precursors ranging from the Prague School of Mathesius through the M.I.T. School of McCawley and Harris) who

have remarked about dividing a vocabulary into native/non-native suncomponents, Saciuk takes loanwords and the other "exceptional" elements as given facts of a language (in this case Spanish, Portuguese and Catalan) as it is attested in the single speaker; he concentrates not on the workings of "nativization," but on the role of different hypothesized native and foreign components of "lexical strata" within a lexicon which has become a fixed synchronic feature. Saciuk 1969 serves, then, like its earlier prototypes, as a proposal for describing the phonology of a target language after the established fact of language contact. However it has nothing to say about what are the actual processes of lexicalization.[13]

Chapter Four of this study in particular departs from generative studies like Saciuk 1969 in taking as its focus the role of the lexicon (i.e. rules of word formation which in a model approximating Halle's give structure to formatives comprising the lexical listings or stored dictionary) in actually restructuring new loanwords in early stages of borrowing.

Some limited attention is also given in Chapter Seven to methods for eventual nativization of foreign segments as they become over time fully a part of the native system. The intimate connections between Natural Phonology, loanword phonology, and the structure of a speaker's lexicon should of course be patently obvious: the structure of the lexicon is product of a system of phonology which itself is revealed transparently by (among other various types of alternations) the manner in which borrowings are handled by the native speaker. After all, when we inquire how processes are active in determing our perception of foreign sounds it is equivalent to inquiring how they determine the

perception of our own native system (see Lovins 1974b). And any complete loanword phonology must deal not only with initial treatment of borrowings in the native system but with questions as well of how foreign elements eventually lose their "exceptionality" and become fully and irreversibly "nativized." It is not difficult to conclude that a study of the processes of borrowing reduces inevitably to a study of the processes of lexicalization. And such conclusions lend weighty support to the theory Stampe espouses: that all elements of phonological systems -- diachronic and synchronic, developmental (child language) and degenerative (apraxia), native and foreign -- derive from the single underlying principle of innate natural processes. Eventually, of course, full and scientific treatment of the lexicon (word formation processes and not just allomorphy) from the standpoint of Natural Phonology will be altogether indispensible, and it is a first step in precisely this direction that I have aimed for with Chapter Four.[14]

Though little has been said to date within Natural Phonology about the subject of lexical representations, certain implications of this theory for the structure of a lexicon are also somewhat inevitable. One immediately obvious distinction between the concept of the lexicon required by Natural Phonology and that in standard generative theory might help in sorting out contrasting tenets of the two theories. This is that whereas in abstract generative phonology the lexicon is over and again employed as a type of repository for all problems resulting from an over rigid simplicity criterion (e.g. an inventory of segments is proposed on the basis of Markedness criteria and features counted, along with principles of symmetry, and the more abstract these segments

prove to be the more complex and numerous become the rules needed to account for them), in a system of Natural Phonology (which assumes processes to be the primitives) the content and organization of the inventory of segments is never analyzable in isolation but instead is always a manifestation of the process system itself (compare Lovins 1973, page 27). Since both immediate lexicalization and eventual nativization result from either forwards application of higher-level processes or backwards application of the lower-level ones, knowledge of the processes of the native system provides a method for both speaker and linguist to arrive at proper underlying forms (a convenience not available to either Skousen or Chomsky and Halle). Implicational laws of Jakobson, Markedness of SPE, redundancy conditions of Stanley, MS conditions of Halle, and surface constraints of Shibatani -- all these reduce to "an innate system of phonological processes which resemble the implicational laws and the markedness conventions in content but have the same ontological status as the native processes (so-called "rules") of the phonological system of any individual language" (Stampe 1973c, pages 44-45). It is in this particular sense, then, that conditions on lexical representation must be dealt with in subsequent chapters. And it is also in this special sense Stampe's notions of processes may be shown to be entirely as relevant in accounting for the nature of a speaker's lexicon as they are in explaining observable surface phenomena of acquisitional phonology, or as they are in describing the seemingly related substitutions which predominate with examples of casual or unguarded speech and even with examples of drunken speech (Lester and Skousen 1974).

1.4 Remarks on Cuban Spanish and Methodology

No assumption is made with subsequent chapters that readers of this dissertation are previously familiar with any precepts of Natural Phonology beyond those few now sketched in Section 1.2. Quite to the contrary, this general unfamiliarity makes Part Two of necessity a lengthy account of Stampe's theory: its workings and its basic motivations.

In order for these initial chapters to be intelligible, however, and in order that familiar positions and controversies of generative phonology (e.g. Markedness, abstractness, extrinsic rule order, and motivations for linguistic change, etc.) need not be entirely restated fresh, it must be assumed that any reader entering here is thoroughly versed in these fundamental "issues" of generative theory. That is, a minimum acquaintance is assumed with the important books as well as articles and circulating papers of at least the following generative phonologists: Chomsky and Halle (especially SPE), Postal, McCawley, and Kiparsky (e.g. his 1965 dissertation Phonological Change and milestone 1968 article "How Abstract is Phonology?").

At least a basic working familiarity with generative treatments of Spanish phonology is also taken as granted. Observations to be made generally about the phonology of the American Spanish dialects are based on the seminal book by Harris (1969) and the dissertations of Foley (1965) and Saciuk (1969) -- all three featuring the Mexico City dialect of Spanish as a standard -- and it is expected that any readers have also absorbed these earlier treatments as background. Since previous works on Cuban Spanish are rare, on the other hand, all claims about these dialects are more fully documented from original

and sometimes rather obscure sources.

Finally, assumptions about phonology in Miami Cuban Spanish (MCS) are drawn from my own field work, as well as from studies which are now in progress or which have very recently appeared. One such work is an ongoing analysis of MCS phonology by Bohdan Saciuk, which has thus far resulted in only one formal presentation (Saciuk 1974) yet promises significant contributions to American Spanish dialectology. The second is a recent dissertation by Robert Hammond (1976) which undertakes the study of certain rapid speech processes of MCS and broaches a discussion of implications of such processes for universal phonological theory. Hammond's dissertation, especially, suggests some crucial topics for debate when we turn to a specific analysis of MCS in Chapter Five and Chapter Seven of this present study.

Data gathered about the Cuban dialects until recent years remained remarkably sparse. And where it was available it was generally less than properly analyzed. Often there are conflicting observations and even contradictory conclusions.

As examples, studies by Isbaşescu (1965, 1968), based one a handful of informants who were Cuban students resident in Bucarest, suggest that a velar nasal allophone appears generally before k and in utterance-final position, though occasionally before other consonants as well. However, in Guitart 1973, which is drawn from informal observations of fellow Cuban exiles in the United States and supposedly represents an Educated Spanish of Havana (ESH), we discover that at least for Guitart's personal dialect (though I am not convinced this is adequate generally even for his own personal idiolect) the obligatory rule is for all closed syllable-final

nasals to be velars. Though Saciuk (1974) notes a strong tendency among Habaneros to voice intervocalic stops, this tendency has received only scattered and fleeting reference in the earlier studies. More recently, Hammond (1976) labels as "rapid speech phonological processes" velar nasal intrusion, intervocalic voicing of the voiceless stops, s-aspiration and deletion, r/l confusion, devoicing of final unstressed vowels, final consonant deletions, spirantization of voiceless stops, monophthongization, and others. Yet monophthongization is a morphological rule rather than a phonological rule and its relationship to rate of speech is at best highly questionable. And concerning most if not all the remaining processes mentioned, while they are undoubtedly more frequent in more casual speech and thus in some sense tied to speech style, still any causal relationship to speech rate would be prohibitively difficult to verify. My own observation is that all these processes are frequent though random in occurrence even in the most guarded style of speech reported.

Chapter Five will treat these issues generally, and in particular the problems of spirantization and what Guitart (1973) and Hammond (1976) both record as velarization of nasals will be scrutinized. To merely hint for the present at what seems an essential inadequacy of Hammond's concentration within a generative phonological framework on "rapid speech phonological processes" cited above, we might observe that spirantization and final consonant deletions, at the very minimum, represent certainly optional and sometimes obligatory rules in the non-casual as well as casual speech for most speakers. To suggest that these "rules" also extend their effects to sloppy or careless speech patterns is only to suggest that they are in reality universal

phonetic processes (in the Stampean sense) and not acquired "rules" at all. Also, that such processes spread their effects to casual speech is only endemic to their character as natural processes (see Section 5.4 below) and not any indication that they might be at all equivalent to an SPE type notion of the phonetic detail rules.

An earlier scarcity of work on Cuban Spanish is recently being replaced with a surprising burst of scholarly interest and energy. In addition to Guitart 1973 and Hammond 1975, 1976 there are two further dissertations of the Spanish of Havana (Lamb 1968 and Sosa 1974) and a limited but steadily expanding list of articles on Cuban phonology. Guitart 1973 remains, however, the only monograph-length contribution on insular Cuban Spanish which merits careful attention with regard to my own study.[15] Of considerable significance as well (though more as a spur to further debate than as any empirical solutions to reigning problems in Cuban phonology) are a series of unpublished and soon-to-be published papers by Terrell (see Terrell 1974a, 1974b, 1975a, 1975b, 1975c, 1975d). These papers, with a theoretical orientation toward Vennemann's Natural Generative Grammar, treat the following topics: (1) functional grammatical constraints on deletion of word-final s in Cuban Spanish; (2) percentages of occurrence and the interrelation of processes of aspiration and deletion of syllable-final and word-final s in Cuban Spanish; (3) percentages of occurrence and interpretation of implosive and final nasal phones; and (4) evidence from the above phenomena in the Spanish of Cuba which lends support to an interpretation by means of Vennemann's natural generative theories. Data utilized by Terrell is from a series of taped interviews of twenty-two Havana natives carried out in Miami as part of the "Coordinated Study

of the Linguistic Norms of the Principal Cities of Ibero-America and the Iberian Peninsula" (Comisión de Lingüística Geográfica, Consejo Superior de Investigaciones Científicas, Madrid).[16] Unlike my own informants, however, none of those referred to by Terrell had been in the United States for more than three months at the time of the interviews.

I will not review the literature on Cuban phonology in any detail in this current chapter, as this has been the practice of most previous dissertations and especially thorough reviews are already available in chapters by both Lamb and Hammond (though Lamb's review unfortunately antedates Terrell's work and a number of other important papers). We might only observe that studies on Cuban Spanish have fallen largely into three distinct categories.

First, there is the major category of studies which consists exclusively of lexical compilations, mostly produced early in the present century. These are of little use to contemporary phonologists yet they have been unfortunately almost a sole basis for a number of earlier phonological studies (or reputed phonological studies). Lamb 1968 gives a brief account of such compilations in its second chapter reviewing the history of Cuban phonological studies.

Second, there are two works of major proportions done from a structuralist standpoint and describing the Spanish of the Havana Province. Lamb 1968 is itself a traditionalist description of phonemic and phonetic inventories with some additional discussion of general characteristics of the Caribbean linguistic zone; Sosa 1974 attempts to relate Cuban phonology to a Spanish Creole and to trace features of a Cuban dialect to the influence of slave populations over the past two centuries.[17] Again, neither study is of any pressing interest to the

currently practicing generative phonologist.

Finally, there are the two earlier-mentioned dissertations which (in the fashion of my own study) relate Cuban linguistic data to broader questions of theoretical interest. Hammond 1976, as noted, examines the rapid speech rules seemingly distinct from ordinary phonological rules through observations from MCS. And Guitart 1973 investigates a claim that language-particular constraints (in this case phonetic neutralization phenomena in ESH) are explained with a theory of generative phonology amended to incorporate the SPE notion of Markedness. Data and analyses presented in these final two works are somewhat crucial to my own assessments of Cuban Spanish and this is a point which will be pressed in detail with Chapter Five. Specifically, Guitart's observation of widespread velarization in Cuban Spanish and Hammond's first account of a spreading spirantization process and the possible implications in terms of restructuring of underlying inventories are both integral (albeit in a largely negative sense) to my own contrary argument for a Natural Phonology analysis of MCS and my thesis that innate processes and not learned rules will account most insightfully for the native phonology and borrowing phonology which together characterize this special and emerging dialect.

Additional claims about MCS and the bulk of the data for this study derive from my own field work in Miami and Gainesville carried out over the eleven month period between January 1975 and November 1975. The entire corpus of recorded material consists of twenty-seven tapes of native informants and Spanish-language radio programming and totals approximately fifteen hours of recorded speech. All taping was done with high-quality Memorex cassette tapes on a Lloyds cassette-style

recorder; however for the purposes of analysis these tapes were either played back through high-quality stereo speakers or listened to through a set of stereo headphones. With the informant tapes, both types of analysis were made. Twelve primary informants were taped in formal or semi-formal interview sessions in either Gainesville or Miami, though all had lived in Miami for a minimum of five years, used Spanish as their dominant language (that is, at least in home environments), and resided no extensive periods outside of Cuba and Miami. Several additional informants were also recorded in the course of gathering data for this study, but as these either had difficulty with the presented questionnaire, showed almost no knowledge of anglicismos current in Miami style Spanish, or had resided in Miami for only several months, their tapes were discarded for the purposes of analyzing this investigation.

These formal-interview tapes were supplemented with approximately ten hours of additional cassette recordings of Miami Cuban radio programming consisting largely of talk-shows, newscasts, and commercial messages, with almost all recorded music having been eliminated. The original purpose of this radio taping was to gain some general familiarity with frequent and phonologically-interesting borrowings as well as to solicit items for inclusion on the linguistic questionnaire. Radio tapes eventually proved a fruitful source of loanword pronunciations, however, and the use of this corpus in the phonological analysis of borrowings in Chapter Seven seems fully justified in light of our assumption that the predominant source of phonetic borrowings is the first renderings by largely bilingual speakers who are in turn imitated by the monolingual members of the speech community. Also, as a bulk of English borrowings into MCS are place names and product names, a

fecund source for determining the original phonological shape in the target language (Spanish) for the borrowed word (a crucial factor in analyzing the course followed toward nativization) is radio commercials and radio news broabcasting. Additional discussion of my field work methods and a list of informants and phonetic forms is provided in the Appendix following this study.

The questionnaire (also see Appendix) used was one designed solely for this project and contains all the following types of closely related interview items: (1) sentences featuring interspersed English borrowings (downtown, bisi "busy", apointment, hacer espí "to hurry", and the like) and to be repeated by the informant after an initial repetition by the interviewer; (2) printed lists containing similar borrowings from which informants were asked to repeat (i.e. read) each word and briefly relate what it meant; (3) numerous sentences of the "fill-in-the-blank" variety to be repeated, with the precise word required by the blank often being largely irrelevant as the items were frequently designed to solicit pronunciation of another word or words in the sentence; and (4) several questions designed to elicit "free discussion" and several printed illustrations (newspaper photos and Norman Rockwell prints) for the same purpose of eliciting free and informal conversations. Each interview was carried out in Spanish and lasted only about thirty minutes. Admittedly, having the informant repeat items already first pronounced by the investigator or read from printed forms often leads directly to questionable results; at the same time, no other approach seemed practical or offered any real hope of eliciting certain desired responses. The shortcomings and built-in inaccuracies of this methodology were kept clearly in view when selecting genuine loanword pro-

nunciations for inclusion within the corpus of transcribed borrowings which comprises the results of this investigation.

It must be emphasized here initially that the discussion of Miami Spanish featured in Part Three of this dissertation is not in any sense intended as complete listings or a complete phonological description of all English borrowings (nor even a representative percentage of them) current in this dialect. There is, of course, no fixed number of loanwords in any dialect where such borrowing remains an active and ongoing process of linguistic change. I have taken as my subject just those processes apparent in borrowings which provide insights into the workings of loanword phonology and justifications for a system of Natural Phonology approximating that outlined first in Chapter Two through Chapter Four. Drawing heavily upon the list of loanword proposals systematized by Lovins (see pages 21 to 23 above) as well as other hypotheses which would seem to arise inevitably from the theory offered by Stampe, I have sought out evidence in Cuban Spanish bearing on such fundamental yet still unverified claims as those in the abbreviated list of loanword hypotheses on pages 8 and 9 of this chapter. Three in particular give shape to Chapter Seven and merit special repetition here:

Hypothesis I: The propensity of a language for borrowing words is to a large extent determined by what shapes are permissible at its surface level -- that is, it is generally late syntagmatic processes and not underlying restrictions (paradigmatic processes or morphological rules) which are relevant in determing what can be borrowed and which operate on lexical items once they are adopted by a target (borrowing) language system.

Hypothesis II: Loanwords have their underlying representations

established as phonemic representations and are nativized through application of allophonic processes, facts which lend credence to the proposals for a "natural phonemic" level.

Hypothesis III: Only the "living" processes, which are fully productive in the grammar and will admit no exceptions, and not the "dead" and unproductive rules, function in initially establishing a loanword in the target language.

A goal of this dissertation, then, is ascertaining whether proposals of this type (introduced with Stampe 1969 and first studied formally in Ohso 1971 while not being explicitly codified until the works by Lovins) withstand perceptive analyses of borrowings into some language other than Japanese and English. And also, whether or not the evidence from loanword phonology is to any important degree a confirmation of more general principles advocated by the Natural Phonology espoused by Stampe.

NOTES

[1]Some clarification would seem to be needed here, given that Lovins's dissertation, as far as it goes, offers an excellent introduction to the various approaches to problems in loanword phonology, including approaches within Natural Phonology (esp. in Section 2.2.2.5). And Ohso's earlier Master's thesis is also a pioneer effort in Natural Loanword Phonology which also repays most careful study.

Lovins (1973) and Ohso (1971) both examine English and (in Lovins) other Western borrowings in Japanese, a data source which has been the only language-contact situation so far to attract the handful of linguists advocating Stampe's approach to loanwords. (This is in large part no doubt due to the fact that James McCawley, the mentor of both Stampe and Lovins, is the prominent student of Japanese among the generative phonologists, as well as the fact that Ohso is herself a native Japanese.)

Yet for all their many merits both these works are only reasonable first approximations at treating loan phonology data within Stampe's methods. Ohso gives sparse treatment to the theoretical implications of her Japanese forms. Explicit discussion of Natural Phonology and Natural Loanword Phonology in Lovins 1973 is likewise restricted to three brief sections of Chapter Two (pages 25 to 37), and this in a thesis which on the whole explores numerous approaches and several plausible solutions to loanword phenomena. Fuller theoretical discussion of loanword phonology in the light of Natural Phonology is to be found in Lovins 1974a and Lovins 1974b.

As long as Natural Phonology remains in its infant stages, it is of course not surprising that pioneer works should reveal such clear limitations. The considerable potential of efforts in Natural Loanword Phonology becomes somewhat more apparent in Lovins's more recent work: viz. in Lovins 1974a, which explores the "stratification" of the lexicon into "native" and "non-native" subcomponents from the standpoint of Natural Phonology (with a conclusion that lexical representations have stratal attributes as a direct consequence of which rules or processes have already applied to them, an interpretation granting to lexical strata "a rather different mode of existence"); and in Lovins 1974b, which presents for tentative consideration the hypotheses outlined above beginning on page 21 and subjected to some further analyses below in Chapter Seven. The sum effect, however, of these earliest attempts is only to emphasize the obvious fact that no extensive or even adequate study of loanword phonology as parallel to Natural Phonology is yet available.

[2]For example, one widely cited and otherwise quite exceptional paper (Kornfeld 1971) has summarized recent tenable theoretical positions in child phonology without even once mentioning Stampe's hypotheses among them.

[3]One of the difficulties with Stampe's present theory is the notion of the form in which the child inherits an innate system. This issue must for the time being remain unresolved. Our only evidence is that demonstrating the unalterable fact of innate processes.

[4]Rhodes (1972) argues differently concerning restrictions on underlying forms in Natural Phonology. It is Rhodes's conclusion that such restrictions are not processes (actual substitutions as opposed to traditional morpheme structure constraints) simply because "the lack of ordering relationships between RUFs (restrictions on underlying form -- PCB) suggests that RUFs are, in fact, simply conditions, all applying at the same level in the derivation, i.e. in underlying forms" and therefore "RUFs are MS conditions" (1972, pages 555-56). But the difficulty is that Rhodes considers only one type of process as real -- the context-sensitive -- whereas it is paradigmatic natural processes which Stampe has proposed as being some kind of "filter" or as restrictions on the lexicon. This confusion in types is taken up more extensively in Chapter Two, especially in Section 2.2 below.

[5]Stampe acknowledges that this position is not at all unique and that Passy, for example, expressed such a view at least as early as 1890. See, e.g., Stampe 1969, page 448.

[6]Lovins gives an example confirming this claim that allophonic distribution is relevant to characterizing the "closest sound" in the target language. Though phonetic [s] occurs in Japanese, any foreign [s] is rendered as phonetic [s] only when it occurs in a context which permits the phonetic [s] in Japanese.

[7]However, Ohso notes that dominant (context-free in her terminology) processes may indeed influence heavily the perception of foreign sounds, and their production as well (see Ohso 1971, page 42; Lovins 1973, page 32). Ohso labels as "dominant" those processes which constrain underlying inventories of segments, and she apparently has in view here only processes of the paradigmatic type ("X → Y eliminating X from the lexicon"). Throughout the discussion which follows and in the model for Natural Phonology given in Chapter Four, I employ the label "dominant process" normally in the same sense, though this may be some distortion of what Stampe originally intended. Stampe has emphasized (personal communication) his own designation of "dominant" refers to any process which applies earlier than a second one and therefore establishes the environment for the second process. The context-free processes are normally always prior to contrary context-sensitive processes which counteract their effects (e.g. general vowel denasalization must always precede contextual vowel nasalization in English). But the syntagmatic processes might also dominate over other later and similar syntagmatic processes, given this original interpretation of "dominant" process.

[8]Lovins mentions at several junctures (e.g. see Lovins 1973, page 27, or Lovins 1974b, page 249, note 9) that this phonemic level of Natural Phonology is not a "uniformly positioned" level of underlying representation. This is a point about which Stampe himself is

not always especially clear -- an oversight which leads to most of the difficulties of interpretation discussed in Chapter Two. Lovins's own definition of the phonemic level in Natural Phonology is worth quoting more fully:

> This "level," in Stampe's view, doesn't really exist per se, being peculiar to each process and each morpheme: it is no deeper in each case than is needed to handle allophonic (contextual) variation. It is therefore defined solely by the interplay of context-sensitive and context-free processes, the former allowing (when "allophonic") surface segments that have previously been disallowed by the latter (Stampe 1972b) (1974b, page 249).

Lovins points out that similar suggestions about a possible phonemic level have recently been given in Schane 1971, Wang 1968, and throughout the works of Kiparsky. I would suggest that Lovins is guilty here of the same misinterpretation as Rhodes (see the discussion in Chapter Two); it is the interplay of allophonic and morphophonemic processes, not context-sensitive and context-free processes, that is operative in establishing a phonemic representation. Once this is seen we are led to the view (presented in Section 3.1.2) that Stampe has something much more heretical (in light of the standard generative theory) in mind when suggesting this view of a speaker's notions of phonemic representation.

[9]Stoel claims:

> Stampe is not explicit about the possible number and range of types of "processes" which might be found in phonological acquisition. Consequently his theory, as described in his own works, is virtually untestable.....Jakobson, in contrast, makes precise probabilistic predictions on the order of acquisition of phonemic oppositions.....Jakobson's theory is more suitable for testing than Stampe's (which makes it, in some sense, a better theory) (1974, page 12).

Ohala also complains about testability in Stampe's position on child language:

> Stampe suggests that children are born with a set of universal phonological processes in their head (sic) which tell them, among other things, to produce their stops without voicing, to have no consonant clusters, to drop h's, etc. The children learn how to speak their particular language by deleting or otherwise modifying this innate list of directions on how to talk. This is certainly possible -- I wouldn't say plausible, though. But this hypothesis is too powerful: there is almost no data that couldn't be explained in this way. There is then practically no way of testing the hypothesis, in which case it is a toss-up whether we accept this untestable hypothesis or some other untestable hypothesis, e.g., "things are the way they are because God wants them that way." Frankly, if it is just between these two, I'd take the latter hypothesis because it even "explains" things outside phonology (1974, page 268).

This view is modified, however, by the following retraction:

> My mistake, apparently, was to take Stampe's "innate phonological processes" to be software constraints (mentalistic controls -- PCB). Stampe assures me that they are the constraints inherent in the human body, i.e., hardware constraints. In this case I have no argument with Stampe's theory -- indeed, it coincides with my own view of the subject (1974, page 271).

Ohala is still troubled, however, by how "hardware constraints can be 'suppressed' or 'ordered'" and he questions what it would mean "to 'order' one innate physiological constraint after another" (1974, page 271). An answer in part is that such restrictions on pronounceability are interpreted to be mentalistic as well as physiological, a point which Stampe has spent considerable time defending in the opening pages of his own dissertation (Stampe 1973a).

[10]Of course it is true that Natural Phonology to date has had almost nothing to say about a third rule type also relegated in SPE to the nebulous category of "phonological" component: morphological or morpholexical (Anderson's term) rules. For possible definitions of such rules see e.g. Halle 1973; Aronoff 1976; Skousen 1972, 1973, 1974; Hooper 1973; Anderson 1974, 1975; and Cearly 1974. Chapter Four of this present dissertation is, as far as I can tell, the first attempt to discuss morphological rules, however superficially, within a treatise on Natural Phonology.

[11]Terrell makes a parallel observation in this regard when he comments that

> The preoccupation of standard generative phonological theories with "competence" contribute (sic) to the difficulty which studies based on this model have had in coming to grips with an overall phonological theory with explanatory adequacy. Explanation has been, in general, relegated entirely to the formal apparatus of a generative grammar with an associated "evaluation metric"..... Traditionally, however, questions of perception, production, and acquisition, i.e., performance, have been considered as explanatory bases for phonological processes (1974b, page 1).

[12]Skousen's approach is discussed at length in Clayton 1974, Chapter One and Chapter Four. Clayton's dissertation is itself one len-thy consideration of proposals in the recent literature to limit phonology to "productive," phonetically-plausible rules and of some speculations about the theoretical implications inherent in such limitations.

[13]Saciuk's approach to the "stratification" of the lexicon has received criticisms for its excessive "abstractness" from some recent sources. Wanner (1972) presses the conclusion that Saciuk's scheme for "lexical stratification" can not be part of the phonological description of any language, since it violates conditions on empirical falsifiability. The essence of the argument is captured in the two following paragraphs:

> Another detrimental implication of the lexical stratification hypothesis has to do with the claim it seems to make about the status of its generalizations within the grammar: It is inconsequential for the speaker's knowledge/usage of his language whether the underlying phonological representations are abstract or not, i.e. whether he has analyzed the data along the lines of lexical stratification or whether he arrived at a shallow phonemicization. Lexical stratification therefore does not make any true predictions, it merely states lexical relations on a static basis (1972, page 12).

> It turns out however that in a description making use of lexical stratification exactly these erratic gaps in the lexicon of a particular speaker will be responsible for the difference between the speaker's having an "abstract" vs. "concrete" underlying representation for an item such as leche. This means that there is no further content associated with the notion of underlying representation if it is the case that it may vary extensively and radically across idiolects. Since the difference in underlying representation is not correlated with any surface difference of comparable impact on the overall grammar the conclusion is inevitable that the concept of lexical stratification rests on non-empirical, arbitrary foundations (1972, page 13).

A second criticism more relevant to present concerns of our own study comes from Lovins (1973, page 156). Lovins argues that "if any of Saciuk's surface forms were taken as underlying (phonemic), which they apparently are in relation to synchronic processes, it would be more obvious that underlying representations have stratal attributes only as a function of what rules and processes have already applied to them" (page 156). The analysis by Lovins suggesting that "for forms containing no exceptions to current phonetic processes, it is precisely the phonemic level at which 'degree of nativeness' is intuited" will come under more careful scrutiny as soon as we turn to loanword phonology proper in Chapter Six and Chapter Seven.

[14] A pressing need also will be for research like that now being carried out by Reimold (1974a, 1974b). This is the effort to define in formal terms what qualifies as a natural phonological rule (i.e. Stampe's processes). Where Stampe is concerned with explaining the nature of substitutions which children and later adult speakers will actually make in arriving at utterances (that is, with the question of "performance"), Reimold (like most generative phonologists) is more interested in abstract properties of formal grammars. Reimold's purpose is to set up structural definitions for certain natural rule types which will also serve to account for phonological processes in a more explanatory fashion (1974b, page 2). While according to the standard view (viz. that of Chomsky and Halle, and Schane) natural rules are presumably "simpler" rules, Reimold proposes to define meticulously natural rules in strictly structural terms, so that it will be apparent from the form alone what type of process (e.g. one of weakening, or assimilation, etc.) a rule represents.

[15] Although Guitart claims to characterize the Educated Spanish of Havana (Cuban but not Americanized-Cuban Spanish) which is his own idiolect, and though Hammond and I treat exclusively Miami Cuban Spanish, we are in the long run discussing intimately related dialects about which many assertions (e.g. Guitart's claim on the velarization of the nasals and obstruents) would be equally applicable. I draw this conclusion largely from Guitart's admissions that his own informants are himself and selected personal acquaintances about whom he has commented as follows: "Although the writer and other Cubans known to him who speak this variety (i.e. ESH -- PCB) have been living in the United States for at least five years and are more or less fluent in English, none shows in his Spanish any noticeable phonological influence from English" (1973, page 69, note 1). In other words, Guitart's informants (to the extent that he uses any) are identical to mine and to Hammond's (at least to speakers from our own samples who are university-educated); that is, they are Cuban who have been living in the United States for five years or more and who maintain Spanish as their first language yet are somewhat fluent in English. It is on the basis of this similarity in our samples that I claim to be justified in advancing criticisms (see Chapter Five) of Guitart's few conclusions about ESH and about educated Cuban Spanish in general.

[16] As noted in the text, Cuban informants recorded in Miami also serve as basis for additional studies: cf. Terrell 1974a, 1974b, 1975a, 1975b, 1975c, 1975d; Vallejo-Claros 1970; and Haden and Matluck 1973, 1974. What is not often pointed out is that all these studies draw upon a single set of several hundred taped interviews recorded during the period 1968 to 1970 by Bernardo Vallejo-Claros under auspices of the aforementioned "Coordinated Study of the Linguistic Norms of the Principal Cities of Ibero-America and the Iberian Peninsula" (directed by Joseph H. Matluck and Humberto López-Morales. A full description of these tapes is given in the Introduction to Haden and Matluck 1974. Although the number of tapes employed in each of the individual studies mentioned above varies greatly, in all cases the informants were natives of Havana and none had been in the United States (Miami) more than three months at the time they were interviewed by Vallejo-Claros. In sum total, the project taped over 400 hours of Havana speech in a series of 454 interviews utilizing 750 informants, but all these "interviews" were largely free and unrestricted dialogues between two informants or the interviewer and a single informant and "secret" recordings of spontaneous dialogue or of more formal lectures or speeches, etc.

[17] Like Guitart, both Lamb and Sosa claim to study the Spanish of Havana but base their analyses on interviews with informants who have been residents in the United States for at least a number of months and often a number of years.

Part Two

The Processes of Natural Phonology

CHAPTER TWO

RULE ORDER AND STAMPE'S NATURAL PHONOLOGY

> Assuming that there is some phonological level more abstract than what has been termed the systematic phonetic level, what are its properties and its relationship to the surface phonetic level? This question, the most basic of phonology, is still without a satisfactory answer.
>
> -- Mary Louise Clayton

2.1 Preliminary Remarks

Stampe's conception of Natural Phonology potentially relieves the contemporary theory of phonology of some of its most celebrated pseudo issues. It seems to provide for the resolution of even such particularly thorny issues as rule ordering (e.g. in so far as it sorts out sequential and linear properties peculiar to both "rules" and "natural processes") and the "abstractness" controversy (see Kiparsky 1968a, Crothers 1973, Schane 1974, etc.) in generative phonology. In the introductory chapter we have surveyed some of these characteristics of Natural Phonology and delimited the field of this present study. In this second chapter and the one following I will now relate in more detail precisely how the Natural Phonology of Stampe resolves controversial issues within the generative phonology inaugurated by Chomsky and Halle.[1]

As suggested in Section 1.2, unique to Stampe's model are a concept of "natural phonemic" representations less abstract than underlying forms in standard generative phonology and a related distinction between the acquired "rules" and innate "natural processes"

of a phonological derivation. By making exactly this distinction between learned rules (the limited case which is imposed by each language on its speakers in the sense that all rules are language-specific) and inherited rules or "processes" (the more frequent case which is imposed by the speaker upon his language in the sense that all processes have a universal physiological or mentalistic motivation), Stampe's version of phonology necessitates a considerable reassessment in generative-transformationalist thinking. It is not quite possible to ignore any longer, for example, that the practice in generative phonology of lumping morphophonemic processes and phonetic processes together as a single set of ordered rules has obscured a fundamental distinction among the processes of phonology and misrepresented unique teleologies of rules (Stampe 1973a, p. 43; Schane 1971; Anderson 1974, 1975). Yet the scarcity of published versions of Stampe's papers and resulting confusion over the precise meanings of several of his claims (e.g. the issue of what is actually "innate" in language and what is ultimately "natural") has deprived this alternative model of considerably greater impact to date and therefore motivates full well the present chapters.

An observation of opening remarks in Section 1.1 was precisely this scarcity of Stampe's published writings. Stampe's formulation of Natural Phonology is mostly restricted to papers read to the Chicago Linguistic Society, and though several of these talks since 1969 have reached print others before 1968 have never been readily available. Rhodes's articles (1972, 1973), on the other hand, stand as the most elaborate attempt prior to the 1974 Papers from the Parasession on Natural Phonology (cf. Bruck et al. 1974) at an objective critique and

an explication of Stampe.[2] Yet unfortunately for natural phonologists, Rhodes's papers with all their insights contain some troublesome misconceptions and inaccuracies which so far have completely escaped any commentary. With the appearance in 1973 of Stampe's own frequently delayed dissertation (which aims to answer "shortcomings brilliantly enumerated by Rhodes 1972 which natural phonology seems to exhibit if the role of contrary innate processes in determining underlying representation is not correctly understood," see Stampe 1973b, p. 1) we now have a much fuller text from which to assess claims in Natural Phonology, and it appears inevitable that numerous improved treatments will now eventually be forthcoming.[3] However, since we currently still lack such treatments, this second chapter aims to contribute to a more extensive debate of Stampe's principles and to a more thoroughgoing review of Stampe's position than that found in previous works. To achieve anything measuring close to this goal it will be necessary to address here precisely those issues which are at present the most lively issues of Natural Phonology and of generative phonology, as well as of linguistic theory at large. These are the familiar impenetrable questions of:

(1) How are rules ordered in grammars of natural languages?

and

(2) What precisely constitutes the underlying representations of grammatical formatives?

In short, what are the degrees of abstractness admissible in phonological grammars, and what are the numbers and kinds of phonological substitutions that speakers make?

In commenting on these issues my focus will be principally on

the interpretation of Stampe by Rhodes and on Miller's subsequent arguments (Miller 1975) against Rhodes's own variations of Natural Phonology, as well as on some central features (the Phonemic Hypothesis and evidence for context-free processes) which I take to be the most often misinterpreted facets of Stampe's otherwise fully explicit proposals.

2.2 Conceptions of Rules and Natural Processes

Particularly unsettled at this point is Stampe's division between rules and natural processes. As a case in point, we note that Rhodes (1973) gives one sample derivation in terms of Natural Phonology which suggests that a "natural phonemic level" separates all "rules", which apply before, from the assorted processes applying after. This is reminiscent (Rhodes's own statements to the contrary notwithstanding) of a classical phonemic level which divides morphophonemic rules which result in surface constrasts from the phonetic rules which do not (cf. Schane 1971).[4] But the proposal is not especially helpful in elucidating Stampe's own suggestion: i.e. that it appears to be a level of "short-term memory storage" (having its reality as the locus of "slips of the tongue") which is the expected point of division between what are rules and what are processes (Stampe 1973a, p. 44). Stampe's more specific claim is that "phonological processes apply after slips occur, and therefore must also apply in the processing of individual segments" (1973a, p. 44); and therefore, by implication, the contention is that rules (irrelevant to such processing) apply only before such slips. Rhodes's division of the rules, when coupled with Stampe's suggestion, makes the strong claim seemingly amenable

to empirical evidence that "slips" occur at the point of phonemic analysis; yet it does so without making altogether clear what Stampe's concept of the phoneme and its representation in generative grammar actually is. Rhodes seems to assume, then, that since processes are sensitive to such slips while rules are not, it follows that processes are all of a single similar kind and all act upon (i.e. follow in the derivation) what Stampe would call underlying (phonemic) forms. Much of this particular chapter is devoted to showing the recklessness of any such proposals.

Miller (1975) claims to refute Rhodes's tentative model for Natural Phonology on the basis of a single example taken from ancient Greek of an "unnatural" substitution (a "rule" of initial r-aspiration) which must apply down among the late natural processes, given its status as a completely automatic and therefore late (low level) process. Miller's complaint is not with Stampe's apparent resurrection of a phonemic level, which would be expected to be a popular objection among most generative phonologists, but with Rhodes's quite arbitrary divisions between rules and processes. Very loosely Miller's criticisms seem justified, in so far as Rhodes's division and descriptions of rules and processes don't match with Stampe's. Yet one of the points I wish to emphasize here is that Rhodes's portrayal of Natural Phonology inaccurately represents details of the theory for reasons that have nothing to do with the example put forth by Miller. Also, Miller's example of r-aspiration in Greek is subject of an analysis which destroys any and all contention that it disproves the general claim that rules precede processes and that processes are "automatic" and hence the "natural" substitutions in the grammar.

Actually, three separate and crucial issues appear to be raised with these two papers (Rhodes 1973 and Miller 1975) and might be best summarized as follows:

(3) Rhodes claims only learned rules occur before the phonemic level and only innate processes apply after such a level.

(4) Rhodes additionally claims some rules (preceding the phonemic level) display characteristics of processes and therefore are to be classified as "natural rules," although rules (learned behavior) and not processes is most assuredly what they are.

(5) Miller counterclaims there are some automatic late processes (after the phonemic level) that are learned and therefore a type of "unnatural" (as opposed to "natural" in Stampe's sense), though nonetheless automatic process.

Therefore, whereas Rhodes suggests rules functioning something like natural processes (e.g. Turkish Vowel Harmony, taken up below) which results in both natural rules and natural processes, Miller's position is that there can be rules (or at least "unnatural" processes) ordered among the automatic "phonetic" substitutions and thus below the phonemic representation. I find it somewhat significant, however, that neither seems overly concerned with investigating what actually it is that corresponds to "phonemic representation" in these sample derivations they present; that is, neither paper gives any evidence such as that found in Stampe's dissertation for how speakers must determine what would constitute true underlying sequences from which they derive (at any rate of speed or in any style) strings like "Is pot good?" (which is Miller's example) or a standard pronunciation of e.g. "electri[s]ity" (Rhodes's sample derivation). Or, for that matter, if the speaker

makes any phonemic analysis at all. This last question, I would want to argue -- not merely the orderings of rules versus processes, which may soon become a non-issue at any rate, given the shakey status of the present assumptions about sequential orderings of rules -- is the single relevant question about any system of Natural Phonology.

I will proceed to demonstrate here that all three issues labelled (3) through (5) are at odds with Stampe's repeated position and hence not appropriate claims about Natural Phonology. Whether or not they are to be taken as correct statements about language in general is an issue we will studiously avoid at present, with evidence favoring Stampe's model being postponed for later chapters. I begin instead from an assumption that the argument of this chapter -- viz. that both Rhodes and Miller to various degrees misrepresent Stampe and Natural Phonology -- is prerequisite to any further discussion of the natural phonologist's model as it is described and in some facets only implied in Stampe's own papers.

In this section I deal exclusively, then, with oversights in Rhodes's and Miller's discussions of Stampe. A further treatment of this topic in Section 2.3 dismisses as well the superficial arguments in the recent literature against the phonemic level as a basic level of linguistic analysis and against the reality of Stampe's governing distinction of "paradigmatic versus syntagmatic" among natural processes. Other issues are deferred until Chapter Three and Chapter Four, where I give fuller explications of what I assume Stampe has intended for a proper model of the phonological component within a workable theory of natural language.

As a point of departure, consider the following responses to (3)

through (5), which we will label here (3') through (5') and upon which we will elaborate throughout the remainder of this chapter.

(3') The claim that all processes follow (are ordered subsequent to) Stampe's phonemic "level" ignores Stampe's own announced position that an underlying representation in Natural Phonology is "phonemic" only in the sense that it is no deeper than what is necessary to account for allophonic variations -- Rhodes's claim therefore does not seem to allow for the deeper morphophonemic processes and rejects altogether the existence of paradigmatic processes, the latter normally applying in unrestricted fashion to govern exclusively the stored or lexical representations.

(4') The proposal for "natural rules" to be contrasted with the "natural processes" results at least in part from failure to take into consideration a full set of defining characteristics of innate processes, like that presented e.g. in a middle section of this current chapter and first tentatively put forth as an explicit list in Bjarkman 1975.

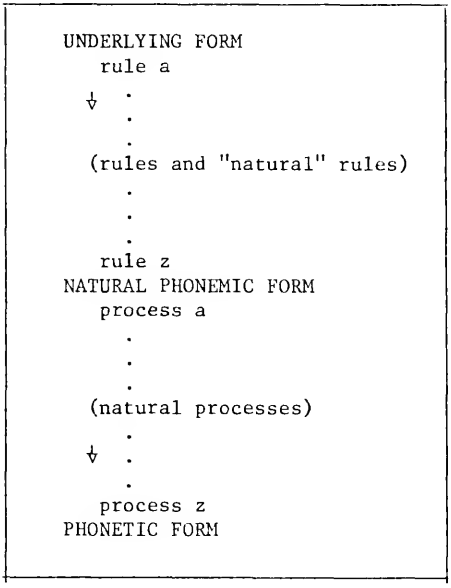
(5') Miller's example from classical Greek will not in point of fact stand up as a valid counterexample, since when the correct historical analysis is supplied the rule in question appears to meet all obvious features of a natural process as opposed to a learned rule -- yet even the proven existence of some undisputed case of the type Miller has in mind would not do irreparable damage to the claim of Natural Phonology, since Stampe does admit (see Stampe 1973a, page 66) the possibility of rare and unnatural cases (and I speculate that these cases might well all involve what are clearly "morphological" rules) where rules might indeed apply after

natural processes.

The model Rhodes proposes might conveniently be labelled as the Sequential Rules-Processes Model (employing my own label) and can be outlined as follows (with capitals indicating levels of representation having some assumed psychological reality and the smaller case letters indicating types of intervening processes).

(6) FIGURE ONE

RHODES'S SEQUENTIAL RULES-PROCESSES MODEL OF NATURAL PHONOLOGY



where: UNDERLYING FORM = what generative phonology calls Systematic Phonemic Form

where: NATURAL PHONEMIC FORM = what Stampe commonly refers to as the "underlying level" but what Rhodes argues is not a Classical Phonemic level

Implicit here is what Rhodes has interpreted as Stampe's claim that no "rules" are ordered down among the natural processes (for some explanation of Stampe's account see Section 3.1.2), as well as an even stronger assumption that all rules precede and all processes follow a phonemic level of representation. With this model it must be concluded that a phonemic representation exists as an autonomous level within a

generative phonology (i.e. there is a line in a generative derivation exclusively equivalent to a phonemic representation), that such a level functions to distinguish rules from processes, and that these latter represent two distinct kinds of substitutions in natural language. The final claim, of course, is unquestionably a claim made by Stampe.

It is useful to review here these assertions by natural phonologists that two distinct kinds of substitutions operate in natural languages. The first and demonstratably most numerous type (subject to the closest scrutiny in literature on Natural Phonology) are the innate processes (often labelled simply the NPs) which have a mentalistic as well as a physiological motivation.[5] Phonological processes function to merge the potential phonological oppositions which are characteristic of speech into a single member of the opposition which most conforms to restrictions on the human speech capacity. That is, into the member (voiceless as opposed to voiced obstruents in final position, for example) which is easiest to articulate. These oppositions are the contradictory sets which reflect the conflicting phonetic restrictions underlying all utterances. As Stampe expresses it, processes attack phonetic difficulties in appropriate and natural ways, but ways which often are in conflict with other potential processes (Stampe 1973a, page 41). Speakers will quite "naturally" devoice obstruents, since their oral constriction impedes an airflow vital to voicing. On the other hand, in voicing environments, say between vowels, a voicing process becomes the "easier" alternative by assimilation. We introduced above Stampe's assumption that a speaker can resolve conflicting processes through the suppression, limitation (a context-sensitive subcase of suppression), and reordering of these processes (Stampe

1969). In its most primitive state, when first exposed in the child, an innate phonological system will display the full array of such restrictions on speech capacity; it is the complete set of unordered and as yet still altogether unlimited phonological processes. The task of the child acquiring a language is, then, to make revisions in all aspects of this system which are at first separating child-like pronunciations from the standard of the adult pronunciations (Stampe 1969). It is a common observation already noted from the literature on child language that many children first produce the English words "dog" and "doll" in identical fashion, as [da]. But once velars are acquired, "dog" becomes [ga], with an apparent assimilation being made to a deleted final velar. (That is, underlying /dɔg/ becomes /dag/ by lowering, then /gag/ by velar assimilation of some type, and then /ga/ by final velar deletion.[6]) Eventually velar deletion is fully suppressed yielding finally the standard output of adult speech.[7]

Through use of the three mechanisms for resolving articulatory conflicts, the child "learns" to pronounce new phonetic oppositions that are revisions in his "innate" phonological system (for many more specific examples see Stampe 1969; 1973a, Chapter One). The mature speech system maintains just those aspects of an innate system that this mastering of adult pronunciations has left intact, along with some learned substitutions which are the "rules," and those processes surviving this more or less "unnatural" sort of limitation of the innate tendencies toward physiological ease will determine what are the pronounceable phonetic representations of any given language. (A process of final obstruent devoicing survives in German dialects but not in English.) Failure to limit processes in this way would

potentially reduce all utterances to the single lax stop and low vowel [ba] with which the child begins. Thus Stampe and his students observe that you in fact can't learn a language (that is, if you are a child) only "by doing what comes naturally." Acquisition becomes a matter of suppressing utterances as much as it is a matter of acquiring articulations.

The second kind of substitution in natural languages is the phonological "rule", which is acquired behavior and accounts for the abstract alternations which often occur in strictly morphological or even grammatical environments. An example appealed to by Rhodes is the English rule of Velar Softening. This is an alternation of /k/ with /s/ and of /g/ with /ǰ/ (/dʒ/) before the reflexes of front vowels in words with Romance origin (e.g. electric versus electricity and pedagogue versus pedagogy), an alternation for which the lexical restrictions (it applies in electricity but not in persnickity) and therefore the learned nature is patently obvious. Stampe (1973a, page 45) significantly observes that children can not be assumed to begin with pronunciations of words like "kitty" and "get" with /s/ and /ǰ/ and later to limit these segments to only words which are the Romance derivatives. Instead, some words are quite obviously learned with /s/ and /ǰ/ in the first place; yet, as Stampe again observes, those forms which are learned from reading sometimes fail to show the expected alternations, and in fact one at times will hear "pedago[g]y" or "fun[g]i" or the like.

To return to Figure One, there are two reasons which should be more or less immediately obvious for rejecting Rhodes's Sequential Rules-Processes Model as the proper description of the type of phono-

logical component Stampe is suggesting. The model taken from the sketch above (Figure One) is inaccurate and misleading, firstly because it ignores altogether a distinction Stampe and the natural phonologists make between two functionally separate classes of natural processes. These are context-sensitive processes (hereafter also called "syntagmatic") which operate to ease articulatory difficulties in sequences of segments, and context-free processes (herafter "paradigmatic") which clarify phonetic properties of the individual segment. A full delineation of the types is given in Stampe 1973a, page 21, and in Donegan Miller 1972a. As paradigmatic processes are predominantly those which will govern underlying representations, Stampe assumes they are necessarily ordered prior to the syntagmatic processes, which will govern consistently surface representations.

It is evident from scanning that Rhodes's model has only one type of process in view and this is of necessity (since they are found following the "phonemic" forms) the syntagmatic variety. More misleading still, Rhodes says nothing about what I would credit as being Stampe's distinction between the morphophonemic syntagmatic processes, which substitute segments not previously eliminated by some prior paradigmatic process (i.e. those not impermissible in underlying form) and thus in the traditional sense "neutralize" underlying distinctions, and allophonic syntagmatic processes, which substitute just those sounds which do not occur in underlying (phonemic) strings. It would seem imperative, then, to take exception to one observation (1973, page 537) by Rhodes in the face of all others: this is a statement that devoicing is a natural process par excellence, and therefore that forms at the natural phonemic level do not show the effects of devoicing,

that bunt "colorful" is represented as /bunt/ (cf. bunte) and Bund "association" is represented /bund/ (cf. Bünde) even though they are together pronounced identically as [bõt^{h}].

A number of points in our discussion below depend rather on the alternative assumption that German final devoicing is a morphophonemic (pre-phonemic) process and thus that German speakers take as phonemically distinct the two forms that Rhodes cites as having identical "phonemic" representations, one fact by the way which accounts well for Shibatani's discovery that speakers take just such a "rule" as a functioning restriction on possible forms of their language (for discussion see Hooper 1975, page 543, as well as Section 3.1.2 below). Such distinction leads immediately to a conclusion that some processes do indeed precede phonemic representation (the morphophonemic type supplying features, derivable but already prevalent in the lexicon, at some level speakers take as underlying) and others (the allophonic ones) do not. And such a conclusion straight away invalidates a model postulating an intermediate level which has only the learned rules coming prior to it and all natural processes ordered after it.[8]

We observed above that Rhodes also argues for a category of "natural rules" functioning above (ordered prior to) the natural phonemic level in particular derivations, a category which consists of some otherwise natural processes which are for some unaccountable reason now operating as rules, among which they are as a result ordered. He would place substitutions like Turkish Vowel Harmony into such a category of rules: i.e. "even though the physiological motivation is clear, nonetheless, in most instances, languages having vowel harmony substitutions are either replete with exceptions (like Turkish), or

contain 'neutral' vowels which obscure the phsyiological motivation (like Finnish, and Nez Perce), or are opaque and strangely constrained (like Yokuts)" (1973, page 534). But since rules which insert epenthetic vowels must apply prior to Vowel Harmony and therefore must also be determined to be of the nature of rules not processes, and since other cases of epenthic vowels are more clealry NPs, it is seemingly necessary to conclude that at least some substitutions must function randomly in given languages as both natural processes and natural-type rules.

Here I would contend that Rhodes is simply guilty of some faulty reasoning concerning teleologies of processes. His mistake seems to be one of equating the naturalness (in the Stampean sense and not the sense assumed by Schane and other generative phonologists) of a rule with "exceptionlessness" of appearance across many diverse languages. Although single processes have transparent physiological and mentalistic motivations, the fact that universality of occurrence is no fast criterion is made clear enough by Miller's case (below) of r-aspiration in Greek. English Flap-deletion is a frequently-cited process in the literature on Natural Phonology but not necessarily a widely-surfacing one among the world's languages. More than likely, the answer to the paradox of Vowel Harmony, a much more straightforward case than Miller's aspiration example, is, I suspect, an obviously historical one. What began as a natural process with all the earmarks of processes (the occurrence in child speech across countless languages, the frequent appearance in adult speech of many languages, and direct physiological motivation) has, by a normal chain of events (see Stampe 1972 on the English Vowel Shift), converted to the learned rule of Modern Turkish.

The case would in fact be parallel to that of the Vowel Shift of English. It is some centuries since such a vowel shift can be viewed, as Kiaprsky (1968a, page 11) erroneously claims, as an automatic and low-level phonetic process; and yet the overly abstract nature of the representations and complexity of derivations in Chomsky and Halle's version of the vowel shift become apparent when one looks at the many non-alternating lexical items of English (see Krohn 1974; examples are the underlying representations for vowels in words like moss, lawn, maudlin). What is a process and what is a rule is not, after all, so difficult to determine once the criterion of function is made the primary measure. Vowel Harmony, replete as it is with such numerous exceptions, simply no longer appears to be taken as a restriction on pronounceability by the contemporary speakers of Modern Turkish.

To recapitulate, I have been suggesting here that Rhodes's formal schematization of Stampe's model is inadequate if only because of its failure to acknowledge a distinction between functionally paradigmatic and syntagmatic processes, as well as a further contrast in types between morphophonemic or pre-phonemic and later allophonic processes. When we turn next to Miller 1975 we discover an attack on Rhodes's schematization which is based on somewhat less substantial grounds than these, yet one nonetheless worth pursuing since it does reveal further potential misconceptions about the structure of a proposed Natural Phonology.

Miller also rejects the Sequential Rules-Processes Model for enticing reasons, though in the act he challenges a tenet of Natural Phonology which Rhodes with good reason has assumed to be a "given" of the theory. This is an assumption that substitutions which are totally

automatic (i.e. exceptionless, "transparent," palpably a restriction on what is pronounceable and thus difficult physiologically to overcome, inviolate in the face of borrowings, "slips," and word games, etc.) correspond to Stampe's set of natural processes and are the residues of what is innate for the speaker and not simply learned arbitrarily, because this or that peculiar language requires certain pronunciations for certain contexts. Miller attempts to argue that "learned" substitutions can also be in this very same fashion altogether automatic substitutions.

To illustrate that there are rules which apply below Rhodes's natural phonemic level, Miller cites the "rule" of r-aspiration which applies invariably whenever this segment is initial in ancient Greek. That "invariably" may be an overstatement of the case is a plausibility we will take up further below but one which need not detain us here. There are superficial indications that this particular substitution displays features of a highly unnatural and therefore presumably learned type of substitution for speakers of ancient Greek. Some of the evidence would be as follows, selecting here only those most salient points of Miller's discussion.

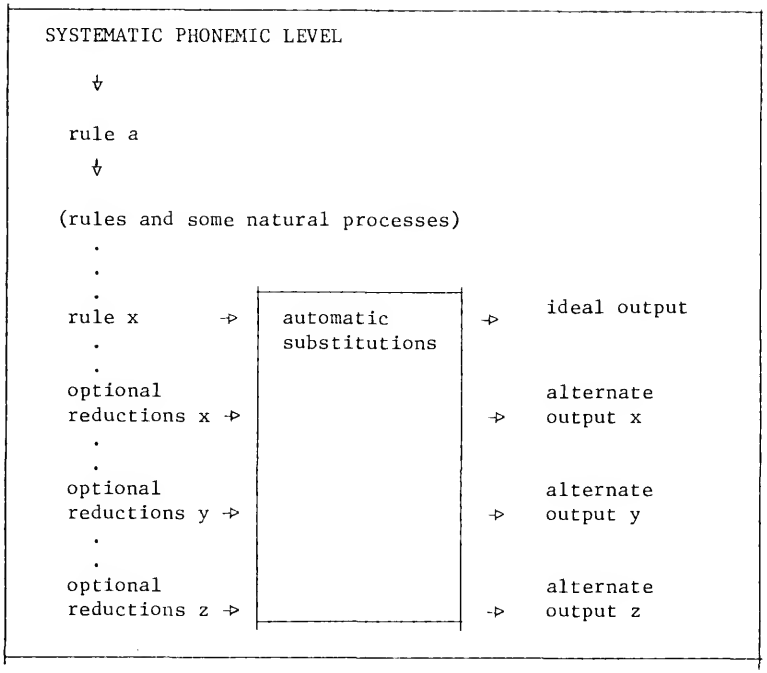
Universally, aspiration is recognized (in the general work of phoneticians like Ladefoged, for example) to be rather infrequent on liquids and nasals. In the specific case of Greek, presumably no sonorant was aspirated in underlying representation and in output strings it is only word-initial [r] that has an aspiration. (We are assuming here along with Miller that orthographic rh represents a voiced segment that is aspirated; that this is not in fact the case will be treated thoroughly below.) Miller emphasizes that r with aspiration is increasingly unusual

in this case since it contrdicts the otherwise impeccable generalization that no voiced segment of this language was aspirated, a fact unexceptionally true of the total lexicon. While an aspirated sonorant would seem to be a highly marked segment universally, early Greek had mh, lh, rh, nh (which contrasted with m, l, r, n) before all were quite predictably lost, excepting the case with the initial r. rh instead became generalized to all initial occurrences, those from sr as well as underlying r.

What is crucial is the ordering of this r-aspiration, which is applicable following at least two natural processes of (1) "aphaeresis" (an optional deletion in fast speech of word-initial short vowels after long vowels and diphthongs) and (2) "ancephale" (an initial vowel syncope), both of which operate to expose r to the aspiration rule (e.g. erēmázo "I go along" → rhēmázo; erōtō̃ "I ask" → rhōtō̃, etc.). Miller concludes that what is more appropriate than a model like that sketched by Rhodes for encompassing such data is the alternative model below, which we might conveniently label as the Alternate Outputs Model for Natural Phonology.

(7) FIGURE TWO

MILLER'S ALTERNATE OUTPUTS MODEL



Wherever the derivation stops in the grammar sketched above, there the automatic substitutions, both natural rules and unnatural rules, are given a chance to apply. The alternate outputs are, then, all possible outputs, depending for their differences on the degrees of increasingly rapid and hence casual ("sloppy") speech. One example would be Stampe's celebrated derivation of the phrase "divinity fudge" (Stampe 1973a, page 59; Lee and Howard 1974) where more than a dozen unique outputs rest on repeated applications of the syllabication, flapping, vowel nasalization, and shortening processes of English.

That some forms in Stampe's derivation are marked starred forms which are unpronounceable because obligatory substitutions have not yet been applied is again a peripheral issue which will not sidetrack us here. We can point to a parallel example as being Miller's own illustration of the phrase "Is pot good?", which could be reduced as far as [spatgUd] following non-automatic complete Cliticization and Copula Reduction, with the application of Aspiration being lost due to the loss of environment for this late process following applications of the above two non-automatic processes. Since the late automatic substitutions (Aspiration, for example) are necessarily given the chance to apply after we reach the immediately pre-output stage (following non-automatic substitutions like Cliticization and Copula Reduction), they patently must apply after a natural phonemic level in the proposed Sequential Rules-Processes Model. Miller's argument in short is, then, that (1) the natural phonemic level as presented by Rhodes is fallacious, since we seem to have a case of an acquired and unnatural rule in ancient Greek following the phonemic level and ordered among late processes; and (2) the natural phonologist (Stampe as well as Rhodes would have to be included) fails to distinguish carefully enough between a block of immediately pre-output automatic substitutions, whether natural or not, and those substitutions which are more obviously physiological in aim. The main point of contention that Miller raises, then, however indirectly it may address Stampe's original proposals, is the potentially devastating issue that all automatic processes do not per force seem to be in any necessary sense truly natural. It is this contention that there may be <u>unnatural automatic processes</u> that must eventually stand up against the weight of empirical evidence.

One difficulty, of course, is with the entirely singular nature of Miller's example. No case built on a single language-peculiar example could be accepted as being more than merely suggestive.

Admittedly, it is fundamental to question any formal organization of the grammar into rigidly prescribed blocks of rules in which these relative orderings mechanically will determine the possible types of extant rule interactions. But Anderson (1974) has already raised this identical issue much more eloquently and with considerable cross-language evidence suggesting in effect that morpholexical, phonological, and phonetic rules are generally but never quite unexceptionally ordered as mutually exclusive sets. The evidence from exotic (Abkhaz, Luiseño, Cebuano) as well as familiar (American English and Danish) languages is that although Morpholexical/Phonological/Phonetic is the anticipated ordering of rule types, it is repeatedly in those cases where the transparency of some morpholexical rule is affected by the presence of a phonological rule that an unnatural ordering is encountered.[9] Anderson nonetheless deduces a general principle: "the rules of the grammar which fall into the three types delimited above, with their distinct formal properties, are inextricably intertwined in the grammars of natural languages" (1974, page 3). The point of Anderson's quite judicious paper is precisely that any principle of ordering faces the realization that natural languages display unnatural rules and orderings as well as natural or expected ones.

Also weighing against Miller's argument is the contrary evidence that r-aspiration, so called, is in fact upon closer inspection reduced to a natural process and appears to be not at all a learned rule which operated in the phonological grammar of ancient Greek. This is most

particularly evident once we bear in mind that the distinction between "rule versus process" is not dependent exclusively on relative orderings in a grammar but more on the identifiable teleologies of the two formal types. In this light there is some indisputable evidence that initial rh is product of a natural process. To accomodate r-aspiration as a natural process in ancient Greek we need only to return to the standard descriptions of the substitution in the accepted handbooks (a point which, in all fairness, Miller was himself in part responsible for suggesting to me). Perhaps also helpful will be some minor though necessary modifications of Stampe's original conception of innate processes. Before we take up such modifications, however, it is advisable to clarify still further the respective teleologies of both processes and rules. It will be such modifications and such clarifications which are not only the major contribution of this present chapter, but ultimately they serve as well as justification for such extensive discussion of the reputed Greek r-aspiration example in the first place.

Above we have established tentatively that "rules" are the learned substitutions. A further characteristic of this type is that they apply to lexical representations to alter categorical values of distinctively specified features (i.e. they are not static conditions or redundancy statements governing the contents of the lexicon but rather have an exclusively post-lexical function); and yet they are not palpable constraints on pronounceability and therefore are quite insensitive to such performance errors as tongue slips.

Furthermore, rules are seemingly never optional ("electri[s]ity" never alternates with "electri[k]ity" in any known form of dialect); they make radical substitutions of segments; they have abstract (grammatical

or morphological but never purely phonetic) environments and they are always context-conditioned; they are probably linearly as well as sequentially ordered (though this is the issue least likely to find easy resolution in contemporary phonology); they quite obviously do not convert through mechanisms of historical change to processes; and, finally, they are not productive in the language in the productive sense (i.e. they do not apply to nativize loanwords, in a fashion elaborated in Chapter Six and Chapter Seven).

Processes, by complete contrast, are an innate residue of the primitive language system. They may in fact determine what is permissible in the lexicon and are in a very real sense restrictions on lexical forms (as with the paradigmatic process which eliminates underlying nasal vowels from the segmental inventory of English or the syntagmatic process which determines that all stops will be voiceless after tautosyllabic s of English). Additionally, processes are confirmed restrictions on pronounceability which can not be violated even by slips of the tongue (i.e. they reapply after such slips); they are often optional in so far as they are affected by speed and the style of speech; they make only quite minimal substitutions; they are manifested as either context-free or context-sensitive (the first type being generally ordered earlier); they have always an obvious phonetic function, given that their purpose is to eliminate difficult phonetic configurations, and what are deemed lexical or underlying restrictions may reapply on surface output (e.g. a restriction like that on English stops after s extends to phonetic output in rapid speech pronunciations of phrases like "let's go" yielding [skow]); they are both sequential and non-linear, as well as random and iterative, in their applications;

they can potentially develop into "dead" rules (two classic cases in the literature being Turkish Vowel Harmony and English Vowel Shift as earlier mentioned); and they do prove to be productive in a synchronic version of the grammar. That processes are not restricted to any linear ordering, but apply and reapply over and again quite randomly each and every time those configurations reappear which they are aiming to eliminate, has been seemingly demonstrated by Stampe's now popular examples of fast speech pronunciations for phrases like "divinity fudge" (1973a, page 55).[10] This principle can without much difficulty be assumed a well-established fact of the phonology -- that is, at least for the distinctions set forth in this present chapter.

We will now consider somewhat further these several defining characteristics of natural processes as I have outlined them here, especially with reference to alleged Greek r-aspiration as one quite exemplary instance. Since the intention here is a clarification of Stampe's principles governing phonological processes, I will limit consideration largely to Stampe's own few repeated examples involving English forms and English rules.

2.2.1 Non-suspendability of natural processes

Even the optional variety of process peculiar to fast speech pronunciations is most difficult to suspend and therefore sensitive to spoonerisms or slips of the tongue (compare Fromkin 1971). Rules, though obligatory (Velar Softening is not an "optional" English rule), are easily enough suspended and have numerous evident exceptions. The initial aspiration of stops in English, say, will unavoidably apply to the results of spoonerisms like [k^hat/ steip] kotch stape or perhaps [t^hat/ sk,eip] totch skape for [skat/ t^heip] scotch tape, and the

palatality of [k] will similarly be adjusted in line with its peculiar environment (Stampe 1973a, page 44). Yet Velar Softening can quite easily be withheld by speakers to allow the facetious pronunciation of "electri[k]ity" or a normal pronunciation of the rare but certainly not unheard of form "persnickity" (see also Hooper 1975, page 544).[11] The complete exceptionlessness of the initial rh in ancient Greek would, then, tend to argue most persuasively for its legitimate status as a full-fledged natural process, insensitive to any notion of suspendability attached to rules.

2.2.2 Optionality of natural processes

Processes, on the other hand, are often only optionally retained, relative to the governing style and the speed of articulation. There is nothing as all automatic about certain "sloppy speech" rules of English: viz. aspiration of voiceless stops initially in the stressed syllable (e.g. [prəpɛ́r] becomes [pʰrəpʰɛ́r] "prepare"), optional consonant unrelease after penultimate nasal consonants (e.g. [hʌmp'] becomes [hʌmʔ] "hump" or [hʌnt'] becomes [hʌnʔ] "hunt"), or syllable internal d-deletion (e.g. [ɛ́ndgém] becomes [ɛ́ngém] "endgame" or [kájndnəs] becomes [kájnnəs] "kindness"), or any other of a number of related processes surveyed in Hill 1973 which are facilitated but certainly not dictated by physiological and articulatory pressures.

Rules apparently will never have this optional property. Once it has been acquired, Velar Softening (adopting this standard example as or prototype of the notion "rule") will apply for each speaker to "electricity", and yet as we have noted it does have evident exceptions in some other forms with exactly the same phonological characteristics (i.e. "persnickity" and "lickity split"). We must maintain a careful

distinction here between what I am calling suspendability and what we label optionality: Velar Softening may be suspended by conscious effort, as it has no apparent irrepressible physiological motivation and represents no restriction on pronounceability (it is no more difficult for one to say "electrikity" than "persnickity"), but it is normally applicable irrespective of style or rate. The exceptionless, automatic nature of Greek r-aspiration, which seems to relate it to physical restrictions or at least presumed physical restrictions of the speech capacity, is again on this count suggestive of its status as a restriction on what is perceived to be pronounceable by speakers of this language. That this is not a real restriction on pronounceability, as we shall see promptly, is apparently of little real consequence to speakers.

2.2.3 Extremity of natural processes

Processes make only minimal substitutions (change only a limited few features) while rules make more radical substitutions (change a greater number of distinctive features of any segment). That this is in fact the case has not always been so readily apparent. Yet where the substitutions that are fully qualified processes appear, on the surface, to make more radical changes in segments, as in the example of Joan Velten's drastic replacements of [l] with [z] in a much-cited case from literature on child language (Stampe 1973a, pages 12 and 63), the case turns out to be one of sequential applications of several distinct substitutions (l → j → ʒ → z), each in its turn actually making only the most minimal potential change. Delateralization in this case functions initially to eliminate a difficult tongue configuration; Spirantization will increase or amplify the audibility of [j] with a sibilant; and Depalatalization replaces fronted and

raised tongue posture with the neutral position of plain alveolars. The functioning of individual processes makes obvious a physical and mental restriction on speech mechanisms that no single and sweeping type change between abstract elements might ever encompass.

2.2.4 Non-linearity of natural processes

Stampe contends that processes are both sequential (they are not simultaneous) and yet non-linear (they may apply and reapply) in their applications. They are never merely static conditions like the morpheme structure constraints or mere formalisms like the redundancy rules but are actual substitutions working upon real segments in the mental and the physical performances of the normal speech act (Stampe 1973a, page 43). This is an issue which merits our much fuller attention.

Stampe's theory assumes not only that "systems of phonological processes are real, that underlying and superficial representations of utterances really exist, and that they are constrained and interrelated by the actual agency of these processes" but also that processes "are actual substitutions occurring in the performance (mental as well as physical) of utterances" and thus able to provide not only analogical but literal explanations of phonological events (1973a, pages 43-44).

But it is the argument that processes are non-linear that is most controversial in Stampe's phonology.[12] This argument is set forth most convincingly in terms of evidence from the several casual speech pronunciations of the phrase divinity fudge analyzed in Chapter Two of Stampe's dissertation. The analysis depends on several successive applications and reapplications of the three processes of Syllabication, Flapping, and Flap-Deletion. It is reproduced below (Figure Three), for the convenience of our discussion here, precisely as it is found

in Stampe's original chapter. Here the derivation is taken up at an intermediate stage and starred forms represent forms which remain unpronounceable until later obligatory substitutions have been applied. Even further reductions than those shown here of course remain possible.

(8) FIGURE THREE

ALTERNATIVE DERIVATIONS OF DIVINITY FUDGE

PROCESSES	STAMPE	LEE AND HOWARD
	[dəvɪ́nəti (fʌ́dʒ)]	[dəvɪ́nəti (fʌ́dʒ)]
SYLLABICATION	1. *də.vɪ́n.ə.ti	1. *də.vɪ́n.ət.i
FLAPPING	2. *də.vɪ́r̃.ə.ti	2. *də.vɪ́r̆.ər.i
VOWEL-NASALIZATION	3. də.vɪ́r̃.ə.ti	3. də.vɪ́r̃.ər.i
FLAP-DELETION	4. də.vɪ́.ə.ti	4. ?də.vɪ́.ə.i
SYLLABICATION	5. *də.vɪ́ə.ti	5. *də.vɪ́ə.i
VOWEL-NASALIZATION	6. də.vɪ́ə̃.ti	
SCHWA-HARMONY	7. də.vɪ́ɪ̃.ti	6. *də.vɪ́ɪ̃.i
SHORTENING	8. də.vɪ́.ti	7. də.vɪ́.i
SYLLABICATION	9. *də.vɪ́t.i	8. *də.vɪ́i
FLAPPING	10. də.vɪ́r.i	
FLAP-NASALIZATION	11. də.vɪ́r̃.i	
FLAP-DELETION	12. də.vɪ́.i	
SYLLABICATION	13. *də.vɪ́i	
VOWEL-NASALIZATION	14. də.vɪ́ĩ	9. də.vɪ́ĩ

where r = English flap

I = front unrounded high vowel (lax)

* = unpronounceable form (i.e. all relevant processes have not applied)

The point of the divinity fudge analysis is quite obviously that processes are nowise restricted to linear order but will continue to reapply each and every time an input configuration will arise. This claim has predictably not gone undisputed, however, and one especially controversial alternative analysis of this derivation is provided most recently by Lee and Howard (1974). In this alternative treatment the reasons for questioning Stampe's interpretation are taken to be something like the following.

(a) Stampe's derivation of divinity fudge and his definition of Syllabication (as a process) are apparently in contradiction. To quote directly: "According to Stampe's own description of the Syllabication process, we would expect the [t] to be attached to the preceding syllable, since the following syllable is unstressed. Such a syllabication would allow the derivation (2) above [cf. Figure Three -- PCB], in which there are several fewer reoccurrences of processes. Stampe's derivation of divinity fudge and his definition of Syllabication are thus in conflict" (Lee and Howard 1974, page 221).

(b) Stampe's formulation of Syllabication does not seemingly capture his original intent. Again quoting here directly from Lee and Howard: "A significant fact about Stampe's derivation (1) [Figure Three -- PCB] is that the gradual contraction of the form is the result of deletions which take place within the stressed syllable. The deletion of [t], for example, comes about only after other processes have operated to bring it into final position in the stressed syllable. We gather from Stampe's discussion that his statement of Syllabication does not correctly capture his intent, inasmuch as he insists that [t] is syllable-initial and his argument rests heavily upon that assumption" (page 221).

(c) It might be assumed Stampe is claiming that a process of Flapping in one example involving /t/ results not from an obligatory syllable-final Flapping process but rather from an optional syllable-initial process which is only tangentially acknowledged.[13] It is just this assumption that is made by Lee and Howard and for them, therefore, "certainly the crucial role of the syllable-structure assumption that is being made merits a clearer statement of this process than we are given" and "it is difficult to see the justification for syllable-initial flapping here, since the environment in the doctor (before a stressed vowel) is quite dissimilar to the one that is relevant in divinity" (page 222).

(d) Stampe's proposed derivation turns out unnecessarily lengthy. Lee and Howard have countered that it is sufficiently problematical that there are additional casual speech processes suggesting a further syllabication of the nonsyllabics between stressed vowels or diphthongs within the preceding syllable. A consequence, therefore, of assuming that /t/ is optionally attached to the preceding syllable is that processes of syllable-final Flapping and optional Flap-deletion result in the forms which Stampe has marked unacceptable. A reanalysis of this data would therefore seem to be in order, and one suggested reanalysis involving motivation of an obligatory process of Vowel Syncope yields derivations of the pronunciation of divinity fudge that are considerably shortened, such as those in Figure Four.

(9) FIGURE FOUR

SHORTENED DERIVATIONS OF DIVINITY FUDGE

PROCESSES	INTERMEDIATE STAGES	STAMPE'S EQUIVALENT STAGES
SYLLABICATION	1. *də.vɪ́n.ət.i	
FLAPPING	2. *də.vɪ́r.ər.i	
VOWEL-NASALIZATION	3. də.vɪ̃́r.ər.i	STEP 3
FLAP-DELETION	4. *də.vɪ̃́.ə.i	
SYNCOPE	5. də.vɪ̃́.i	STEP 12
SYLLABICATION	6. *də.vɪ̃́i	
VOWEL-NASALIZATION	7. də.vɪ̃́ĩ	STEP 14
SCHWA-HARMONY	.....	
SHORTENING	.....	

SYLLABICATION	1. *də.vɪ́n.ət.i	
FLAPPING	2. *də.vɪ́r̃.ər.i	
VOWEL-NASALIZATION	3. də.vɪ̃́r̃.ər.i	STEP 3
FLAP-DELETION	4. də.vɪ̃́.ər.i	STEP 4
SYNCOPE	.....	
SYLLABICATION	6. *də.vɪ̃́ər.i	
VOWEL-NASALIZATION	7. də.vɪ̃́ə̃r.i	STEP 6
SCHWA-HARMONY	8. də.vɪ̃́ɪ̃r.i	STEP 7
SHORTENING	9. də.vɪ́r.i	STEP 10

where r = English flap

I = front unrounded high vowel

* = unpronounceable form (all relevant processes not applied)

If the derivations given in Figure Four are reasonable (a question on which we might elect to suspend final judgment at least at present) then Stampe's instances of reapplications of processes in this example are considerably reduced. But nonlinear applications have obviously not been strictly ruled out, since two processes of Syllabication and two of Vowel-nasalization will still apply. Yet Lee and Howard also have reason for doubting that even these instances provide any sound argument in support of the case for nonlinearity as Stampe maintains. One argument stems from the distinct types of English Vowel-nasalization involved in these separate applications, while another views Syllabication as a universal rather than a language-specific condition on rules.

In the case of Vowel-nasalization, for example, one application is progressive while one is regressive. This means "for these to constitute a valid argument against linear ordering, we would have to claim that these form a single unitary generalization" (page 224). Lee and Howard continue further in this vein: "While it could be argued that we are offering no evidence against such an assumption, there is good reason to be suspicious of mirror-image environments constituting unitary generalizations. Aside from Lightner's admonition (1971:234) that the mirror-image convention predicts linguistic changes of unattested types, evidence may be given from both synchronic grammars and children's acquisition to show that such cases often (if not always) have quite different properties" (page 224).

If the two occurrences of nasalization in Stampe's derivation might be examples of quite distinct processes once more closely examined, then processes of Syllabication, after the fashion by which Lee and Howard assess them, might well also represent distinguishable types. Since

the application involves both a gliding of the vowel and an attaching of this vowel to an adjacent preceding syllable, and since Stampe has earlier indicated Desyllabication and subsequent attachment to the previous syllable appear as a single-step process aimed at avoiding the untenable condition of an unpronounceable stage in a derivation (either a syllable with no syllabic segment or one with two syllabic segments), Syllabication in the sense of this second type application would perhaps be better described as simply a type of automatic "fix-up" process which is a mandatory resyllabication following gliding that has resulted in an ill-formed syllable without its proper syllabic segment. Where gliding would be a language-particular fact of English, resyllabication would stand in the nature of a constraint universally applied to grammars or a surface condition on all types of syllable structure. "Like 'prunning' in syntax, this resyllabication principle would apply whenever its structural description is met in any derivation of any language, without exception" (page 224). To quote from Lee and Howard still further: "Real processes change pronunciation, but we doubt that the mere presence or location of syllable boundaries reflect actual differences in pronunciation, though there may be consequent phonetic changes" (page 224).

The point of reviewing the several arguments presented by Lee and Howard against Stampe's analysis is to make absolutely lucid a proposal that such arguments do not in fact rule out nonlinearity; nor do they by themselves establish linearity as a condition on processes required in any grammars. Lee and Howard choose instead to view the single derivation Stampe has provided as being "no more than an abbreviation for a set of nine derivations" though at the same time they sense

Stampe's treatment is still somehow neater. The end result finally turns out to be a conclusion that "although Stampe based his argument against linear ordering on divinity fudge, he provides one, more solid, piece of evidence elsewhere" (page 227).

The example in question is the fast-speech pronunciation of the English word hitting (or perhaps sitting or sinning). To capture more formally a vague sense that Stampe's notion of derivations is somehow sounder than their own, Lee and Howard attempt to formulate a principle which they label, for lack of a more descriptive term, as Misasterism or "the hatred of stars." The notion here is that in the progress of any derivation all the starred forms (unpronounceable stages) are to be eliminated as promptly as possible.[14] The more formal thesis is one requiring that optional processes and subprocessesdo not apply at all to starred forms. The most direct outgrowth of a formal principle of Misasterism is that the number of unacceptable forms in the derivational history of any particular utterance will be markedly reduced. Lee and Howard suggest as a case in point that Stampe's pronunciation [dəvĩr̃əri] will, under another set of quite different conditions, result from only one application of the crucial process of Flapping. "However, if Stampe is correct in saying that the [n] must be flapped but the [t] needn't be, the form with two flaps must be derived from [dəvĩr̃əti]" (page 225). Adopting the formalism of Misasterism (or any similar universal condition on grammars which says that obligatory processes work on the unacceptable forms in derivations to assure that new steps in derivations are constantly yielding pronounceable forms), requires, then, that there be two applications of Flapping and as a result no strictly linear ordering for a process like Flapping.

The similar case with hitting therefore provides a close parallel. Here, only when Flap-Deletion has applied preceding a nasal segment (to [r] before [ĩ]) will the vowel before the deleted flap now also be nasalized. Flap-Deletion yields [hĩ́ĩ̃ŋ] from [hír̃ĩ̃ŋ]. Essentially, "since the second vowel is obligatorily nasalized, but Flap-Deletion is optional, Masasterism would require two applications of (regressive) Vowel-Nasalization in the derivation of [hĩ́ĩ̃ŋ]" (for fuller discussion of this given derivation see Lee and Howard 1974, page 227).[15] But the derivation of hitting is parallel to the derivation of divinity in still another sense, and even here Lee and Howard have expressed a considerable reservation about potential conterexamples to principles of linearity. It is still possible, they suggest, that nasalization on the first vowel of a form like sinning [sĩ́ĩ̃ŋ] may be greater than that on the initial vowel of a companion form like sitting [sĩ́ĩ̃ŋ] (that is, the two might not be homophonous). Again, then, a Vowel-Nasalization process following Flap-Deletion may be an altogether different one from that preceding Flap-Deletion. Lee and Howard would conclude from this that Stampe still has not altogether established the case for non-linearity.

What is vital here is that the literature in the field is not without numerous other examples of processes which apply and then reapply whenever their structural descriptions have been met. Another especially good example involving the Russian treatment of obstruents has been supplied in Rhodes 1973. This involves forms which undergo an often discussed Russian rule (= process) assimilating the voicing of obstruents in clusters (viz. Halle 1959). Here we can uncover several applications of this process to such underlying forms cited by Rhodes as /at-žéč bi/ "were one to anneal" and /tavárʲišć bi/ "comrade" (emphatic).[16] In both

cases, as they are reproduced below in Figure Five, it is necessary to assume two separate applications of Voicing Assimilation and therefore that the process is always available to reapply whenever its structural configuration might be met. This is just another way of saying that a process once it has applied can never be irretrievably lost and unavailable to later stages of the same derivation, provided its proper environment again arises.

(10) FIGURE FIVE

NON-LINEAR APPLICATION OF RUSSIAN VOICING ASSIMILATION

DERIVATIONAL STAGES	"were one to anneal"	"comrade" (emphatic)
UNDERLYING FORM	at-žéč bi	taváŗišč bi
VOICING ASSIMILATION	adžéč bi	tavárišǰ bi
VOICING ASSIMILATION	adžéǰ bi	taváŗižǰ bi
OTHER PROCESSES	adžéǰ bɨ	taváŗižǰ bɨ
SURFACE FORM	[aḍžéǰ bɨ]	[taváŗižǰ bɨ]

In one sense, the situation pictured for Russian obstruent voice assimilation in Figure Five is very much parallel to the cases discussed by Lee and Howard providing two applications of Vowel Nasalization in hitting and two (or more) applications of Syllabication in divinity fudge. The fact that there happens to be no intervening processes here between the two consecutive applications of Russian voicing assimilation seems to be of no real relevance to the issue of repeated and hence not strictly linear processes. It is also evident to one familiar with Halle's classic statements on phonemics in generative grammar that an argument might again be constructed after the model established by Lee and Howard to suggest that the repeated applications of voice assimilation here are in fact (like the cases from Stampe's treatment of divinity) applications of two distinct and not at all identical processes. To arrive at this we would only have to note that one assimilation is "allophonic" in its effects while the other is "morphophonemic". The initial application of voice assimilation following underlying /at-žéč bi/ (i.e. t → d) introduces a form that already occurs (i.e. is not banned by an earlier context-free process) in underlying sequences; a second application (č → ǰ) introduces one that does not. For Stampe, recall, "morphophonemic" processes (t → d in Russian) introduce in the course of derivation segments already found at the level of lexical storage (or what the "standard" theory would deem underlying); "allophonic" processes (č → ǰ in Russian) are those with a strictly phonetic role and which thereby introduce only derived segments. But again it is questionable if this is at all relevant to the issue of distinct process types. For "allophonic" and "morphophonemic" as Stampe uses them (and as they have been employed traditionally in phonology as well) are merely descriptive

labels having to do with identifying the effects of processes; they are not relevant at all to the essential nature of processes. That is, whenever the structural description is appropriate, a phonetically-motivated substitution (if the speaker has not learned to suppress the process in question) will take place; this regardless of the fact that essentially the same process may have one effect on the overall pattern of the grammar in case one, by merely substituting one permissible underlying segment for another, and another effect later in case two, by introducing an allophonic feature not stored in the lexicon. It is the nature of the underlying inventory and not any change in the nature of a process like assimilation which marks one application as "allophonic" and yet another as "morphophonemic".

A fuller discussion of the contrastive functions and ordering of the allophonic and morphophonemic processes, which is a defining element of Natural Phonology, must be postponed until subsequent chapters where I take up more exhaustively the issue of the "phoneme" as Stampe seemingly conceives it. It is worthwhile noting here, however, that Rhodes's second example from Russian of the phonetic form [tavárɨžj bi] (Figure Five) is of special interest and difficulty since the two applications of voice assimilation are in this case an allophonic process (č̬ → j̬) preceding (contrary to the apparent notions of Natural Phonology) a morphophonemic process (s̬ → z̬). The implication of such an ordering must be reserved for the section (3.1.2) of Chapter Three where I have attempted to define more precisely the concept "phonemic" as it applies in Natural Phonology. The value of such a derivation here is only that it seemingly serves as still another lucid illustration of what is essentially and identifiably the same process recurring in nonlinear fashion each and every time its

appropriate environment has been reestablished.

If examples from Russian voice assimilation or English nasalization are not sufficient illustrations closing out the case against claims for linearity among natural processes, one additional example underscoring the complexion and function of a distinction between "allophonic" and "morphophonemic" among rules and processes of grammars might also be given. This final example is drawn from Catalan and is one employed by Koutsoudas and Dinnsen to strengthen a claim for universal or intrinsic (versus grammar-specific or extrinsic) ordering relationships among the rules of grammars.

Koutsoudas observes that in two separate contemporary dialects of Catalan (Western Catalan and Central Catalan) there is distinct ordering for processes of Spirantization and Terminal Devoicing (Koutsoudas and Dinnsen 1976). In the more conservative Western dialect of Catalan the Spirantization process is ordered first and therefore blocks Terminal Devoicing from applying. The standard and more innovative Central dialect shows derivations which reveal Spirantization applying <u>after</u> Terminal Devoicing and as a result being bled by Terminal Devoicing: this ordering yields a surface form [əβrig əspəlifat] "shabby overcoat" from underlying /əbrig əspəlifat/, where Spirantization of the final segment in the initial word has clearly been prevented by the earlier devoicing of this segment.

Such contradictory orderings in closely related dialects might at initial glance seem sufficient evidence that ordering of rules or of processes is indisputably a grammar-specific issue. This is another way of saying that there is extrinsic linear ordering among processes. But what Koutsoudas and Dinnsen aim to expose is that consideration of

all relevant data seems to support a somewhat different conclusion: in the Central dialect of Catalan Terminal Devoicing, just as in the Western dialect, retains a morphophonemic effect and neutralizes underlying contrasts; Spirantization, on the other hand, has become in the Central dialect an allophonic process due to loss in the underlying inventory of constrasts between /b/ and /v/. In part, Koutsoudas accounts for this discrepancy in both the underlying inventories and surface phonetic realizations in two closely related dialects with a hypothesized universal principle governing rule order: the pronciple of Morphophonemic Precedence requires that morphophonemic rule applications always take precedence over (occur before) allophonic rule applications (Koutsoudas and Dinnsen 1976, page 8). The thrust of this example is that the restrictions on relative orderings or processes (Process A must precede Process B in dialect of language Z) is a matter of universal principles (especially allophonic and morphophonemic results of certain processes) and not of idiosyncratic features of individual grammars.[17] Another way to put it is that if there are restrictions on the ordering of some processes, they do not seem to result from strict requirements of rule linearity, either language-peculiar or universal.

2.2.5 Phonetic functioning of natural processes

Processes are by nature a clear reflection of their phonetic function, in a fashion that rules obviously are not. One way to state this is that rules most frequently provide relationships between sets of morphemes while processes act to constrain articulations. What is "unnatural" then from a phonological point of view is for any process not to apply, to by some means be blocked, if there is something available to which it might have applied. Rules, on the other hand,

display highly cognitive characteristics (which appeal to a speaker's knowledge of such grammatical paraphernalia as alpha conventions, minor rule features, brackets, and miscellaneous lexical conditions, etc.) which phonologists have too often unwittingly assumed to be plausible as well for processes, as in one case discussed at length by Stampe (1973a, page 47) of the diachronic English process which has been misapprehended often as a rule of so-called "trisyllabic shortening" (it is actually the result of a simple change of requirements for shortening from doubly to singly closed syllables). There should be little or limited argument that r-aspiration of the type cited by Miller (whatever turns out to be its ultimate interpretation) has undeniably phonetic properties and not obtrusely cognitive properties.

2.2.6 Allophonic influences of natural processes

As a final relevant distinction between learned rules and processes, it can be observed that rules along with the type process we have been labelling "morphophonemic" are all operative as constraints determining shapes of strings of words or morphemes in phonemic representation, while allophonic processes never have this feature.

This is not to say that such rules affect the underlying "inventory" of segments, which is generated exclusively by the context-free or "dominant" type process. That rules should exert such an influence results directly from the fact of grammars that the phonological rules, along with the unordered paradigmatic processes and the morphophonemic processes causing neutralizations of existing underlying segments, all will apply prior to any level of representation conceived of by speakers as "phonemic". Hutchinson (1973), who argues for some formal constraint on grammars that would provide a universal principle of ordering which

is independent of specific grammars, provides a revealing interpretation of how and why precisely this ordering must be the norm.[18]

Hutchinson's constraint follows from a series of related yet distinct definitions of rule types. First, phonemic representations are to be seen as those phonological representations which will obey certain constraints in relation to phonetic representations, and among these must be listed Chomsky's (1964) four phoneme constraints of linearity, biuniqueness, local determinancy, and invariance. (The relevance of these constraints to phonemic theory is taken up in Chomsky 1964 and Hutchinson 1972 and is treated briefly below.) Secondly, any phonological rule (Hutchinson is not distinguishing here between rules and processes) which causes violations of any among the phoneme constraints is by definition a morphophonemic rule. Finally, those rules whose effects are not morphophonemic are by definition and by fiat the allophonic rules.

Once we establish such definitions, certain categorizations of rule types are inevitable. A rule or process which converts one underlying segment into another would of course violate a principle of biuniqueness, providing both segments might potentially occur in an identical environment. A rule of metathesis would clearly violate Chomsky's principle of linearity, in that surface manifestations of two adjacent phones would now no longer occur in the same arrangement as their associated underlying phonemes. Such rules would then be morphophonemic and not allophonic; that is, they would manipulate and rearrange strings but never add to the inventory of segments comprising the phonemic representations.

What is directly relevant to the type distinction between rules and

processes which is our subject here is the notion that any substitution making reference to grammatical information will generally violate a constraint which Chomsky has designated as local determinancy ("in computing between phonetic and phonemic representations, or vice versa, the computation statements must refer to information in the input representation"). (For evidence that such violation takes place, the reader is referred to the discussion in Section 2.3 below.) The violation of a local determinancy constraint in these cases means for Hutchinson that such rules are to be taken as morphophonemic in effect and therefore unaffected by the phoneme constraints operating later in the grammar. However, this reference to grammatical information by the operation of any phonological rule within the view of phonology espoused by Stampe is exclusively the defining characteristic of a rule versus a natural process. We have here, then, another means of approaching the issue of why it is that although processes may both precede and follow a level of phonemic representation, learned rules are always prior to such a level. Since they violate the condition of local determinancy, one of the primary phoneme constraints, phonological rules, in the sense that Stampe delimits them, can never apply to representations the speaker senses to be phonemic. We will find later that this is also an explanation for the exclusive role of allophonic processes in the nativization of most foreign loanwords.

Violations of the underlining segment inventory are introduced within a derivation, according to Stampe's analysis, only through truly allophonic natural processes. Once the process status of Greek r-aspiration can be assumed, such a requirement goes a long way toward elucidating Miller's observation that r-aspiration and for that

matter sonorant aspiration in general is unexceptionally absent underlyingly for Ancient Greek.

Now that we have completed a sketch of the essence of phonological processes viz-à-viz the phonological rules of a grammar, we are now prepared for a more searching analysis of Miller's proposed rule of Greek r-aspiration, as well as a number of related examples which at first blush might seem to provide some form of counterevidence to the claims in Natural Phonology. I would like to conclude this section, therefore, with a brief commentary on each of the following loosely related points:

(11) The example of r-aspiration brought forth by Miller as a challenge to the notion of processes normally following rules turns out on closer inspection to be a non-issue, since the so-called aspiration rule is revealed through inspection of the standard handbooks to be a misnomer and the more appropriate analysis offers no difficulty whatsoever for the principle that natural processes regularly follow after unnatural rules.

(12) A seemingly more forceful example (of the very type Miller has in mind) of an automatic yet unnatural rule ordered to follow the fully natural processes might be adopted from Schane's (1971) analysis of familiar French denasalization rules, but again more searching analysis obviates what had first appeared as a serious counterexample.

(13) Perhaps the most forceful argument now in print that the phonological rules and the phonetic rules come freely intermixed in grammars (Anderson 1974, 1975) bears no relevance to Natural Phonology, as it draws no comparable distinction between what are processes (innate) and what are rules (learned).

(14) What is "natural" in the terms of Natural Phonology is often no more than what speakers impressionistically believe to be the physical restrictions on the pronounceability of their languages and not actual physiological restrictions at all.

Elsewhere (Bjarkman 1974a, 1975) I have argued extensively yet not very adequately for the naturalness of r-aspiration based on pattern evidence in ancient Greek (the "exceptionlessness" of initial rh, the minimal change in phonetic features, the "phonetic quality" of the rule) and appeals to cross-linguistic data (the appearance of an aspirated r or pharyngeal continuant in the rapid speech data of Cuban and Venezuelan Spanish dialects and in Parisian French as well as a regular aspiration of initial r in Welsh). That this proposed analysis is as much lacking as Miller's original analysis becomes patently clear, however, when we make careful inspection of the standard sourcebooks on the phonology of Classical Greek. I will quote several passages at length here, since they seem to put to rest all further debate surrounding the issue. The first passage is from Sommerstein's (1973) Sound Pattern of Ancient Greek.

> The unvoiced liquid [r̥] of Ancient Greek. When initial, or when geminate, or when it follows an aspirated consonant, /r/ is realized as an unvoiced liquid. We can have no hesitation in identifying this liquid as a realization of the phonological unit /r/. It shared all the phonetic features of [r] except voice, and it was in complementary distribution with [r]. Historically, doubtless, [r] and [r̥] were derived from quite distinct sources [the evidence is provided in Allen 1968, pages 41-42 -- PCB]; but there is no reasonable way of deducing this from the facts of classical Attic.....One alleged exception to this rule [Sommerstein's proposed rule of so-called R-Devoicing -- PCB] is perhaps worth discussing: the name Pâpos Râros with its derivatives (see Allen 1968, 40). It has been suggested that the voicing of the initial /r/ in this word is due to the presence of another /r/ at the beginning of the next syllable; but this is unlikely for the following reason. There is another Greek word beginning /rVr/, namely pwpós

/rōrós/, a rare adjective preserved for us by Hesychius' lexicon (5th cent. A.D.).....If Pâpos Râros had a voiced initial, it can have had nothing to do with the phonological environment [my emphasis -- PCB]. More likely the ancient grammarians believed that the name was derived from the dialectal noun papos raros (cf. Allen loc. cit.), in which the voiced initial /r/ is a dialectal feature and the accent shows that the first vowel was short.....If such a belief existed, then the alleged voiced intial of the name could well be an invention of ancient scholarship. At any rate it can not be taken as established that when the Homeric Hymn to Demeter was recited at Athens or Eleusis the initial /r/ of PAPION Rârion (line 450) was voiced (1973, pages 47-48).

A second and parallel passage is from Allen's (1968) Vox Graeca.

Generally speaking [r] is a voiced sound, but in certain environments in classical Attic it seems to have been voiceless. What we are actually told by the grammarians in that p was aspirated at the beginning of a word, and that when a double pp occurred in the middle of a word the first element was unaspirated and the second aspirated (e.g. Herodian, i, pp. 546 f. L). These descriptions are followed in the Byzantine practice of writing initial p̓ and medial p̓p̓, and are supported at an earlier period by Latin transcriptions such as rhetor, Pyrrhus.....As Sturtevant (page 62) has suggested, we may probably interpret this variation as meaning that the aspiration neither preceded nor followed the [r], but was simultaneous with it, i.e. that the sound was "breathed" or voiceless [r] (all aspiration in Greek, unlike Sanskrit, being voiceless). Dialectal support for such a value has been seen in the modern Tsakonian development of [ši-] from Laconian p̓i-, though this also suggests a fricative pronunciation of p.....Such a sound is found as a distinct phoneme in e.g. modern Icelandic hringur "ring" (contrasting with voiced [r] in ringur "gust"); but in Greek it was merely a contextual variant, or "allophone", since initial p was regularly voiceless (my emphasis -- PCB). The only exception of which we are told by Herodian is the name Pãpos and its derivative (loc. cit., cf. also Choeroboscus, Schol. in Theod., ii, page 43 H); the reason for this exception may be that the following syllable begins with p, but another word p̓ápos is also cited by a scholiast on Dionysius Thrax (p. 143 H) as Aeolic.....and the non-aspiration is here explained as being due to the dialect (of which "psilosis" is a characteristic feature)..... It should be emphasized that the voiceless pronunciation of p in certain environments is a purely allophonic matter, and no confusion can therefore be caused if p is always pronounced with its voiced value, as e.g. in modern Greek (1968, pages 39-41).

If we now resurrect Miller's original argument, we recall that its most serious challenge to the schema of Natural Phonology is

that we have the case of a highly "unnatural" even though perfectly automatic and exceptionless rule (r-aspiration) ordered in the grammar after two indisputable natural processes ("aphaeresis" and "ancephale"). This in turn follows from the conclusions that the graph rh represents exclusively aspiration (a rare and unstable phenomena with respect to sonorants), that no Greek sonorant is aspirated underlyingly, and that in Classical Attic no voiced segments may be aspirated. A supporting argument would seem to be that the "learned" rather than "natural" status of rh would be verified by the occasional exceptions like Pâpos Râros and pápos in which aspiration is not evidenced. But such an argument collapses in the light of accounts of initial voiceless [r] in Ancient Greek similar to those quoted above. The heavy "breathy" quality accompanying the production of initial or geminate voiceless sonorants or those following other aspirates is an expected and automatic result of the increased articulatory effort involved with such segments. And there is no contradiction remaining in the general Greek propensity for aspirating voiceless segments -- all aspirates of Greek (opposed to e.g. Sanskrit) being unexceptionally voiceless. Finally, the devoicing of [r] in initial position by speakers of this ancient dialect, which seems actually the rule at issue (clearly we are dealing with two natural processes and not a single process here), is also a most natural phenomenon constraining articulation (as any language teacher attempting to teach English-speaking children to pronounce the initial voiced alveolar trilled [r] of Spanish might be able to verify). It is also worthy of note that with most South American dialects of Spanish the voiceless fricative [ɹ̥] is a frequent replacement for the tap [r] in word-final position, while lengthened

or doubled voiceless varities replace [r̄] word-initially (Dalbor 1969, page 129). I would suspect that other languages will also reveal a highly-similar phenomenon.

But we might still raise a question quite legitimately about the spirit rather than the letter of Miller's argument. Is it not possible to uncover cases of fully automatic substitutions which are nonetheless exceedingly unnatural and which would therefore dictate that Natural Phonology make a finer distinction between automatic processes and natural processes? I know of only one classical case in the literature which would bear examination.

It is a standard fact of French phonology (Schane 1971, page 504) that the surface nasalized vowels of this language have developed from sequences of oral vowel followed by nasal consonant, when followed by a second consonant or pause, and that diachronic rules are still reflected by this allophonic process of modern standard French. It is alternations like [bɔn ami] "good friend" with [bɔ̃ kamarad] "good friend", or [divinite] "divinity" with [divɛ̃] "divine", which prompt Schane to recognize the derived status of all the nasalized vowels in any synchronic grammar of contemporary French.

In order to proceed with our discussion here, it is necessary to recognize two further facts of French phonology. First, nasalization was at one time a more highly generalized process with all vowels nasalized before nasal consonants. Subsequently, nasalized vowels were denasalized when followed by a nasal consonant plus vowel (these were eventually the only VN sequences remaining, since another historical rule deleted nasal consonants before a pause or other consonant). This of course does not necessarily mean speakers of French

ever acquired an active denasalization rule per se, but rather that the rule of nasalization became less general, applying only in environments where following nasals themselves preceded a consonant or pause. The second fact of considerable relevance here is that there are exceptions to the generalization that nasalized surface vowels occur only before a pause or before segments other than nasal consonants, and Schane offers the following examples of nasalized vowels preceding intervocalic nasal consonants: "the indefinite article un ([œ̃n ami] un ami 'a friend'), the adverb bien ([byɛ̃n eme] bien aime 'well liked'), words such as [ãnɥi] ennui 'boredom', etc." (1971, page 505, note 2).

The denasalization process of Schane's analysis is clearly a diachronic phenomenon and I know of no overwhelming support for any analysis featuring a denasalization process in the synchronic phonology of contemporary French. Since this analysis also remains a possibility, however, let us consider several of its implications, particularly as they relate to the question of ordering among automatic and natural rules.

We might speculate, for example, that Denasalization operates as an automatic though somewhat unnatural process in contemporary French (it makes a minimal substitution of the type which characterizes processes and which is attested across languages in children's speech and appears to be a more or less exceptionless restriction on pronounceability for speakers of French), but such a proposal would leave us with precisely the case Miller has in view: an automatic but still largely unnatural substitution following after an attested natural process of contextual vowel nasalization. As Schane also comments

(1971, page 504), it is extremely paradoxical at best that such a process of nasal dissimilation should follow immediately on the heels of a contradictory natural process of nasal assimilation, in the identical environment to boot. (Note here that children's spontaneous denasalizations are normally context-free, and Stampe's notion of contradictory processes does not encompass adjacently ordered syntagmatic ones.) One counterproposal to this entire analysis, of course, is that the substitution in question is after all not automatic (there being not only Schane's examples cited above but also a case given attention in Picard 1974, page 127 -- perhaps restricted to Canadian French -- of the determiner [mɔ̃n] which contrasts with [bɔn]). Such a proposal would argue against an interpretation of denasalization as even an automatic process, let alone a natural process.

We might then choose to view Denasalization as a learned rule, which would also be problematical to the extent that we would now have a rule following a process, something strictly ruled out by the interpretation already given of Natural Phonology. This is in part the analysis offered by Picard (1974), who stresses that Schane's proposal of a nasalization rule and a nasal consonant deletion rule for French turns crucially on the presumption of a final schwa, which in fact has disappeared from modern French. With the disappearance of underlying schwa, we are left with a conclusion that nasalization is a moribund and unproductive process in contemporary French. Picard continues: "Such a morphologically-conditioned rule, if indeed it exists, is highly restricted.....it appears to involve some adjective alternations of the type 'bon bonne' /bɔ̃ ~ bɔn/, a few determiners

like 'mon' /mɔ̃ ~ mɔ̃n/ (and not the difference between /bɔn/ and /mɔ̃n/), some verbs such as 'prends ~ prenez' /prɑ̃ ~ prəne/ (where vowel alternation causes additional problems synchronically), and alternations brought about by derivational morphemes such as in 'convention conventionnel'" (1974, page 127). Picard's assumptions about the status of the rule of nasalization involve a further refutation of Schane's abstract analysis: "In sum, the loss of final schwa can be said to have turned most nasal vowels of French into underlying segments. The majority of word-internal nasal vowels have always been underlying segments, since they have never alternated with oral vowels anyway, e.g. /grɑ̃/ 'grand' ~ /grɑ̃d/ 'grande'. If Modern French still retains a nasalization process, then it is very restricted by morphological and syntactic constraints" (pages 129-30).

The interpretation of French nasalization by Picard, assuming that it is in any sense correct, seems to rescue the position of Natural Phonology on at least several fronts. I have argued independently in Chapter Four that decidedly morphological rules of the type just described are features of a "passive grammar" in adult speakers, which applies only in the analysis of new and unusual forms which demand an activation of this word-formation component. This is an analysis due at least in part to Halle (1973). Such morphological or word-formation rules are therefore irrelevant to the ordering of blocks of rules and blocks of processes and may be inserted at all points in a derivation. On the other hand, if the nasal vowels of French are underlying nasal vowels (a proposal also of Tranel 1974), and if Denasalization can be assumed for the moment to be an active synchronic process of French (a possibility which we have seen is not very likely), we are now no

longer faced with an ordering paradox. Contextual denasalization does not now follow contextual nasal assimilation as, under the new proposal, there is no active process of nasal assimilation. Though we would still wish to maintain the unnaturalness of Denasalization (the pronunciation or oral vowels adjacent to nasal consonants certainly would require increased articulatory effort), Denasalization no longer must be taken to follow any natural process in the phonology. Of course the most reasonable proposal would seem to be that $\tilde{v} \sim$ vn alternations may well occur in the word-formation component or lexicon, as "via-rules" (Tranel 1974), mitigating any arguments for the synchronic reality of Denasalization as well.

The topic of the preceding paragraphs has been the notion that rules (substitution processes) might be conceived of in distinct "blocks" or types which maintain a fixed ordering relationship: more explicitly, the learned and thus more cognitive "rules" apply as an entire set before the application of the innate processes which restrict pronounceability. Since one additional recent article (Anderson 1974) has been addressed exclusively to this issue, it also would seem to bear some further notice. The thesis of Anderson 1974 (republished with few changes as Anderson 1975) is that rules assuredly are subject to a formal classification as identifiable types -- morpholexical, phonological, and phonetic -- and that these types are inextricably intertwined in natural languages. There are for certain ordering relations, in Anderson's view, but these derive from such naturalness relations as rule transparency, maximized applicability of rules, and distinctness perservation. As verification, Anderson calls forth a number of examples of morpho-lexical rules following phonological rules and

of phonological rules ordered after phonetic rules. As established above, however, the erratic ordering of morphological ("morpho-lexical") rules will represent little difficulty for (and in fact are an explicit feature of) a model of Natural Phonology as elaborated in Chapter Four, which assumes a "passive" word-formation component. Yet ordering paradoxes of the second type -- phonetic rules before phonological rules -- could prove damaging to tenets of Natural Phonology should this division of phonological/phonetic correspond to the division rule/process. Upon examination it does not; all ordering paradoxes of this type in Anderson 1974 involve exclusive sets of processes or strict sets of rules. Such is the case e.g. in the required ordering of vowel-length determination before t/d flapping (both processes) in the English derivations of rider and writer; the ordering of vowel fronting before nasal assimilation (both processes) in Danish; and the ordering of vowel raising before vowel epenthesis (both processes) in Breton.

It remains only to comment briefly on one final feature of the division rule/process, and for this we turn once again to the data from Ancient Greek. The probability that speakers of ancient Attic could utter with little difficulty the few isolated words (the examples above from Sommerstein and Allen) containing voiced and thus unaspirated initial r's is not at all sufficient against a natural process. It is, however, one further example of the need for clarification and even modification of Stampe's original definitions of rule types. Though all natural processes have physical motivation and/or mental motivation which is based on physiological motivation, what seems to count is only what speakers believe are restrictions

on pronounceability. Speakers of English believing that initial pt or bn clusters are unpronounceable normally don't utter them, but the rapid speech pronunciations of words like potato or banana belies the purely physical basis of such constraints. If all natural processes were truly physiological restrictions in some purest sense, then we would have to succumb to the position that the appearance of such processes in one language and not in another (e.g. final devoicing of obstruents in German but not in English, aspiration of initial voiceless stops in English but not in Spanish) would be totally and also unaccountably unprincipled. It is the value of the Greek r-aspiration example that it makes this "impressionistic" feature of the natural processes quite evident.

2.3 On Paradigmatic and Syntagmatic Processes

The previous section has dealt at length with two recent invalid discussions of Stampe's notion of derivations in a system of Natural Phonology. Rhodes's proposed interpretation of Stampe ignores crucial distinctions between types of syntagmatic processes (morphophonemic and allophonic) and therefore cannot fail to distort Stampe's claim about a "natural phonemic" level.

A second paper (Miller 1975) attempts to provide counterevidence against the interpretation that processes or automatic substitutions generally and even language-universally apply later in derivations than do rules, which are the exceptional and thus non-automatic substitutions; yet it fails in this endeavor due to lack of any truly relevant examples.

Though ordering per se is likely not to be the essential defining characteristic of processes, it yet remains that there appear to be

no valid or indisputable instances in the literature where genuine phonological rules will apply to the outputs of automatic process-type rules. A substantial reason why learned grammatical rules must precede at least the more numerous allophonic-type processes will be detailed later in this present section, when we take up more extensively the contrast implicit in Stampe's view of phonemes versus Chomsky's original interpretation of formal phoneme constraints. Word-formation or morphological rules might, of course, be exceptions to such a principle of ordering, in so far as rules with this function may indeed intervene among the natural processes; the model presented in Chapter Four, however, also provides for this erratic application of these word-formation rules without any serious violation of Stampe's essential claims about the nature of phonological derivations.

Before elucidating further the workings of Natural Phonology (Chapter Three) and providing a revised picture of derivations in a Natural Phonology (Chapter Four), I will attempt here to dismiss with this section two potentially damaging lines of argumentation against Stampe's original model. One of these will be what I take to be the insupportable position, still maintained in some quarters, that Halle and Chomsky long since have ruled out any possible appeal to a phonemic level of analysis in phonological derivations. An opinion seems still to prevail, without any apparent real substantiation, that a phonemic level or third level of distinction representations is not only without use but in fact logically impossible in a generative grammar (see Johns 1972). A second target here is a possible notion that English might provide at least one particular case involving interpretation of nasal segments which would effectively rule out language-particular constraints

as potentially being high-level context-free natural processes.

In Chapter One we surveyed in summary fashion the fundamental claims of Natural Phonology and determined the most basic hypothesis to be one contending that restrictions on the inventories of available underlying segments result from active and universal processes rather than from abstractions like "Implicational Hierarchies" (Jakobson) or morpheme structure conditions (Chomsky and Halle). Two far more controversial ancillary issues seemed to be: (1) that the syntagmatic processes are to be taken as two distinct types -- the morphophonemic processes which establish (and thus precede) a phonemic level of distinct representations, and the allophonic processes which act upon (and thus follow) such a level; and (2) that paradigmatic processes evident in the child's earliest grammar may also persist in adult speech, continuing to eliminate arbitrarily segments from the adult lexicon.

The difficulty with the second claim is inevitably that while evidence for context-free processes (what Wojcik 1976 calls "eliminating" processes) is truly abundant in the child's acquisitional phonology, the effects of such processes will often lend themselves to other possible explanations in adult phonology and thus go obscured or even obliterated. Both claims are quite obviously vital to any reanalysis of loanword phonology, which will be the subject of later sections of this dissertation.

It is these latter two controversial claims of Natural Phonology which in fact motivate precisely the kind of arguments against Stampe's model which I hope to dismiss as vacuous. Both claims naturally rely on empirical validation of some sort for their ultimate justification,

and with later sections (esp. Section 3.1.2) I will argue that existance of a phonemic level in psychologically-real grammars has substantial confirmation in experimental studies. At the same time, paradigmatic processes, though they may be patently unverifiable in any direct ways, are supported by a plethora of arguments and evidence of a more indirect kind. But first it is advisable to examine the exclusively theoretical arguments -- one against the phonemic level per se and one against the interpretation of paradigmatic processes as "lexical filters". My purpose here, then, is initially to show that neither of Stampe's proposals can be discarded with any lasting justification on the basis of these purely theoretical and hence speculative grounds alone.

The Phonemic Hypothesis of Natural Phonology, explored in Stampe 1968, is the hypothesis that a traditional phonemic level, which is an intermediate level of representation dividing morphophonemic from allophonic-type processes, and not the systematic phonemic level, which in SPE separates the phonological rules from redundancy conditions, is the basic level at which phonological representation is constrained. Once morphophonemic processes are defined as being those which neutralize the underlying contrasts, then this reduces to another way of saying that the phonologically-neutralized features are to be represented in the abstract grammar as though the neutralizing process had already applied to them.[19] For example, the English homophonous forms lapse and laps (either none or verb) would be phonemically analyzed indentically by speakers as /laeps/, with Progressive Voicing Assimilation having already applied to the plural morpheme of laps (noun) or 3rd singular marker of laps (verb), both of which would be stored as z at the lexical or systematic phonemic level.[20] The question

of empirical evidence behind such a proposal will be postponed for the moment until Chapter Three. Instead, the issue here is simply whether such a phonemic level should be rejected out of hand on what are purely formal grounds. Halle's (1959) and Chomsky's (1964) now classic arguments for systematic phonemics would have very little point, after all, in light of any weighty empirical evidence that an added level (and therefore increased complexity) brought by phonemic representations was, in essence, quite necessary for psychologically-real grammars. But it is also increasingly evident that these purportedly infallible theoretical arguments are now not nearly as defensible as once was everywhere assumed.

The argument by Halle (1959) reduces to a notion that inclusion of any phonemic level complicates a grammar in ways that are counter-intuitive, especially since a Russian Voice Assimilation rule (i.e. a natural process) would have to be applied in one part of the grammar for the alternation t/d (since these phones are both phonemic) and then repeated in another part for the alternation č/ǰ (since ǰ can only be derived from c in Russian therefore is not underlying of phonemic). Either the rule must be expressed twice (a blow to the economy metric) or, on the other hand, there is no level that corresponds to anything we would call "phonemic". Details of Halle's argument are widely enough cited elsewhere and familiar enough in generative theory to need no lengthy discussion here. (For details, the reader can see Johns 1972; Hutchinson 1972, 1973; and Schane 1971.) Hutchinson (1973) and Schane (1971) independently establish that Halle's argument can not stand against phonemics <u>per se</u> but only against the convenience of incorporating phonemic representation into phonological derivations.

Hutchinson (1973), especially, is determined that Halle's position is untenable, if only because it rests on what are now unacceptable conclusions about sequential derivations and about the need for what are rule-specific constraints.[21] It would seem necessary only to amend that, among other deficiencies, Halle's argument fosters the unsatisfying proposal that for speakers of Russian the phonemes t/d are differentiated to no greater degree and in no more drastic manner than are the allophones $\overset{v}{c}/\overset{v}{j}$. For further details the reader is referred directly to the articles already cited.

My own response to the generative denial of the "phoneme" will ignore Halle's treatment and take up instead the line of argumentation found in Chomsky 1964. This choice is seemingly appropriate in so far as Chomsky's treatment borrows from and even restates Halle's original position, provides a far more general or universal consideration of the issues at stake in phonemic representation, and is almost equally as widely-cited as the standard case against the phoneme by the proponents of the generative position.

The intent of Chomsky 1964 was to demonstrate that the various constraints imposed by classical phonemics on phonological representation are too strict and thus rule out numerous potentially reasonable analyses, suggesting at the same time that such analyses and the level of abstraction on which they are based must be dispatched from generative theory.

Essentially the argument reduces to something like the following three related stipulations: (1) the phoneme is formally defined as the segment type allowable in the phonological representation of morphemes when and only when a precisely defined set of "phoneme constraints"

have been met by such representations; (2) some otherwise preferrable analyses of phonological data violate these phoneme constraints; and therefore (3) if such desirable analyses are to be admitted in phonology the constraints must be somehow modified or rejected and hence the phoneme as we have defined it must also be rejected. Some shortcomings in Chomsky's original definitions of the phoneme constraints have been eliminated by Hutchinson (1972), whose revised version of Chomsky's definitions is sufficient to demonstrate that despite these revisions there are still in fact commonplace violations of each of the four central phoneme constraints.

Violations of the Classical Phoneme Constraints

Linearity (defined as: "if phoneme A precedes phoneme B in particular phonemic representations, then the manifestation of A must precede B at the related phonetic level") is inevitably violated by any and all cases of metathesis. A single example from Hutchinson (1972, page 8) to which we will return immediately is the adjunction of verbal suffix /ta/ with the Korean stem /talh/ "hot".

Biuniqueness (defined as: "sets of phonemic and phonetic representations must be in a one-to-one correspondence") is violated by neutralizations like German final obstruent devoicing or Russian obstruent voicing before adjacent voiced obstruents. E.g. German /bund/ "association" realized as [bʊ̃t^{h}] while the plural form /bunde/ is realized without devoicing (choosing an example already cited).

Local Determinancy (defined as "in computing between phonetic and phonemic representations, or vice versa, the computation statements must refer to information in the input representation") is violated by cases like Fanti tone assignment (Hutchinson 1972, page 14), where

tone must be determined by noun and verb categories which can not in turn be computed by merely inspecting the phonemes (and therefore rule out this condition).

Invariance (defined as "each phoneme is associated with a set of defining features which must appear in every manifestation (allophone) of that phoneme") is violated either by cases like the English word can't [kæ̃t] (since phoneme n would here be realized as zero) or by alternations like English opaque/opacity (since phoneme k would here be realized in one form as k and in the other as phoneme s).

Now all these violations of the phonemic principle, as Hutchinson has already effectively demonstrated, occur only if we are bound to an assumption that phonemes have status as the appropriate entities for representing morphemes. That is, they arise only when we are restricted to two levels of representation in our model, as generative phonological theory would demand. If we might for any independent reasons (the empirical evidence is not at issue here) be able to motivate a grammar of the type Halle (1959) has peremptorily rejected, with a level of phonemic representation somewhere between the morph(ophon)emic and the phonetic levels, then we suddenly encounter transparent solutions to the problems posed by conditions of linearity, biuniqueness, and invariance. It does not seem to weaken the point being made here that a solution to local determinancy will have to be handled by a different route not relevant in any sense to the issue of levels of representation (see Hutchinson 1972, page 25).

Consider these partial derivations, for example, all adapted from Hutchinson (1972), who though he is not a proponent of Natural Phonology does capture refreshingly similar views of phonemic representation.

(15) Korean Metathesis (Word-Formation Rule)

(a) morphemic	talh + ta	stem talh	"not"
(b) phonemic	talt ha		
(c) phonetic	talt ha	"it is hot"	

where metathesis does not violate a condition on linearity, which is now relevant only between "levels" (b) and (c).

(16) German Final Devoicing (Natural Process)

(a) morphemic	bund	"association"	bunt "colorful"
(b) phonemic	bunt		bunt
(c) phonetic*	bunt		bunt

*phonetic representation of final segment only

where a neutralizing process like Final Devoicing of underlyingly distinct obstruents no longer presents a violation of biuniqueness, which is also now relevant only between "levels" (b) and (c).

(17) Spanish Prothesis (Word-Formation Rule)

(a) morphemic	skolar	"scholastic"
(b) phonemic	eskolar	
(c) phonetic	eskolar	

(18) English Velar Softening (Phonological Rule)

(a) morphemic	opek	(opaque)	opek + iti (opacity)
(b) phonemic	opek		opaesiti
(c) phonetic	owpeyk		opaesitiy

where neither Spanish Prothesis nor English Velar Softening can any longer be taken as a violation of the condition on invariance, the defining features of each phonemic segment (b) appearing in every phonetic manifestation of that segment (c).

We should observe that allophonic variants in opaque and opacity are accounted for under a condition of local determinancy and admit no difficulty for the point at hand. More important, substitutions applying here between stages (a) and (b) in Korean, Spanish, and English examples are what we have been referring to as the learned phonological or morphological type rules, which by implication at least are always prior to phonemic representations (though not necessarily to all processes) in Stampe's notion of a phonological derivation. Since any such rule, making reference as it must to grammatical or morphological information, will obviously violate a constraint on local determinancy while at the same time exerting a morphophonemic effect, it would seem by definition to be necessarily pre-phonemic. While a Spanish rule of prothesis evidently has origins as an innate phonetic rule (cf. Elcock 1960, page 25), such a rule has surely been "morphologized" in modern Spanish as the type of word-formation rule discussed in Chapter Four. The process applying between (a) and (b) in the German example, by contrast, is again the classical example of a Stampean natural process. Here, in fact, is a most lucid example that natural processes (morphophonemic ones) do after all precede and establish the resulting phonemic forms, a fact about grammars not apparent from either the model presented above from Miller or the earlier proposals by Rhodes.

That a considerable majority of the phonological rules in a grammar are the phonological processes which have a physiological or at least a mentalistic cause is certainly uncontroversial by now. To assume that substitutions of this nature may be taken as distinct process types with distinct roles -- morphophonemic and

allophonic -- is not to depart very far from traditional theory (from traditional American Structuralism at any rate), though it is something of a heresy within the view of generative phonology. Of course, we are suggesting here a level of linguistic reality which must eventually meet the test of empirical evidence; but it is one at least, as we have seen, that can not be effectively rejected on purely theoretical grounds alone.

This is no minor point, in light of the exclusively theoretical arguments which make up the entire inventory of criticisms levied at the phonemic principle. Stampe (1968, page 2) summarizes adequately the status of early phonemic arguments: "Thus it was possible for Chomsky to give an unanswerable critique of phonemic theory simply by showing that the analytic criteria were inadequate in principle .....no empirical evidence had to be cited against the significance of phonemic representation, because none had ever been cited for it."

Also demanding some empirical support, but not so vulnerable to attacks based on simplicity criteria as the notion of phonemic level, is Stampe's claim for the type of process that applies unrestricted to strike from the grammar each and every occurrence of certain arbitrary segments -- those segments taken by speakers of the language to contain some combination of features interferring with ease of articulation or ease of perception (i.e. acoustics). Where we find Stampe's version of paradigmatic natural processes being rejected, the argument has been generally of a much less formal sort: i.e. such devices should not be admitted in the grammar simply because they are not empirically verifiable.

Any claim for paradigmatic processes is, of course, tied to the

claim for the distinction "morphophonemic versus allophonic" in processes (the Phonemic Hypothesis), since "allophonic" processes are by definition (in Stampe's framework) any processes introducing sounds already eliminated by the prior paradigmatic substitutions. English has the general and contextually unrestricted substitutions

(19) Nasal Develarization

$$ŋ \rightarrow n$$

(20) Vowel Denasalization

$$\tilde{V} \rightarrow V$$

in some sense precisely because it has as well contextual rules allowing such segments to be re-introduced in just those environments where there is a physiological demand, viz.

(21) General Nasal Assimilation

$$[\ +\text{nasal}\ ] \rightarrow [\ \begin{matrix}\text{@anterior} \\ \beta\text{coronal}\end{matrix}\ ] \ /\ \_\_\_\ [\ \begin{matrix}+\text{obstruent} \\ \text{@anterior} \\ \beta\text{coronal}\end{matrix}\ ]$$

i.e.

$$n \rightarrow ŋ \ /\ \_\_\_\ g$$

(22) Vowel Nasalization

$$V \rightarrow [\ +\text{nasal}\ ] \ /\ \_\_\_\ [\ +\text{nasal}\ ]$$

When we recall along the same lines that nasality on vowels represents some difficult (highly marked) feature, while spreading of nasality through assimilation of segments is highly motivated by physiological principles, where it occurs, contradictory substitutions are not at all extraordinary.

The remainder of this chapter, then, is devoted to a type of formal argument that has been (or might potentially be) raised against the notion of such context-free natural processes as representing some sort

of linguistic reality and that seems based on reasoning or evidence which is no more substantial than that directed against the phoneme.

We have touched sporadically above on a few of the compelling examples which argue for the reality of context-free processes in child language (e.g. see note seven of the present chapter). Readers wanting more substantial proofs can find dozens of examples throughout Stampe's dissertation. Of course, we will have to keep in view here a distinction between those paradigmatic processes which are truly conditions on underlying representations and others (the examples from Stampe 1973a, page 22) which are, for instance, aimed at strengthening one "compromised property" of vowels (palatality or sonority etc.) at the expense of another and which consequently make no automatic and unfailing elimination of all cases of some segment from the language. Salus and Salus (1974) provide a partial inventory of the context-free processes among first acquisitions of children in several languages; and Donegan Miller's (1972a) thesis presents additional extensive examples gathered from over two hundred languages of how limitation and suppression of a small collection of such processes (Neutralization, Neutral-Vowel Lowering, Palatal-Vowel Unrounding, Nonpalatal-Vowel Rounding, Round-Vowel Depalatalization, Nonround-Vowel Palatalization, Low-Vowel Unrounding, Low-Vowel Depalatalization and Raising) generate vowel systems in the world's languages.

Yet when we examine exclusively the grammars of adult speakers, the status of paradigmatic processes becomes less certain at the very least. That they do not show up directly in all but a very few examples is not in itself the least surprising in the light of Donegan Miller's (1973b) observations that, in the main, adult speech could

hardly tolerate the massive neutralizations of individual segments which would be the result of several paradigmatic processes applying in feeding order. Yet such processes are assumed to apply in the adult system and are a reasonable way of accounting for the vowel systems that exist: e.g. the three timbre class vowel system of a language like Bororo can be determined to result since the Neutralization, Lowering, and Raising processes are being suppressed but Depalatalization and Unrounding are applying to affect the low vowels; and if Raising and Lowering are also allowed to operate then the simpler two-height system in Amahuaca is the result (Donegan Miller 1973a, page 153). The challenge comes in the verification of such a hypothesis; and Donegan Miller admits her own methodology is largely to speculate about the processes involved, given the extant vowel system under analysis, and then to search for confirming substitutions of this type in child language, historical change, synchronic alternations, and loanword phonology (but not in the production of the vowel systems themselves, there being no direct and obvious method of confirmation).

We might want to conclude at this point that we are left with both a strong and a weak hypothesis concerning paradigmatic natural processes. Let us facilitate the discussion by formulating them loosely as follows:

STRONG HYPOTHESIS DEFINING PARADIGMATIC PROCESSES

These processes like all natural processes make actual substitutions occurring in the performance, both mental and physical, of human speech (Stampe 1973a, page 44).

WEAK HYPOTHESIS DEFINING PARADIGMATIC PROCESSES

Context-free processes governing underlying forms are to be viewed

only as "potential" substitutions (Stampe 1973a, page 19) which are perhaps best taken as "statements" ("conditions" or perhaps at best a formalism for a "list") about the content of the phoneme inventory of some Language L.

Stampe's own position from the beginning will admit only a Strong Hypothesis relative to paradigmatic processes; the respective teleologies of distinctive types of processes on which the system is in large part based -- paradigmatic processes maximizing distinctiveness of the individual segments and syntagmatic ones minimizing the sequential difficulties of utterances -- would have little explanatory value without an assumption that such processes were "real" and not merely "analogical". But the only tangible evidence as against speculative evidence that Stampe can seem to muster for the continued role of paradigmatic substitutions as actual constraints on segment inventories in the adult grammar is the argument from loanword phonology. Foreign words, especially French ones, with nasalized vowels generally are denasalized by English speakers (Stampe 1973a, page 18). But perhaps the best known example from Stampe's papers is the proposed alternative treatments through "adaptation of loans by analysis" and "adaptation of loans by synthesis" taken by English speakers to the pronunciation of a foreign name like Nguyen [nujen] with its unacceptable (not permissible in the native system) initial velar nasal. Viewing velar nasal segments as impermissible phonemically or phonologically for English, Stampe here assumes the English speaker has two avenues of analysis, which result in his inevitable pronunciation as either [ŋgujen] ("loans by analysis") or [nujen] ("loans by synthesis"). In the first case he performs a "desperate

analysis" and assumes that [ŋ] must be a derived sound and that it results from the same native processes as [ŋ] in a word like engulf. (I will save a full explanation of such a derivation and its implications for discussion in Chapter Seven.) To avoid such a complex analysis when encountering [nujen], the speaker may instead choose the second avenue of analysis by applying the native paradigmatic constraint in (19) and lexicalizing the foreign word as /nujen/, an acceptable underlying native form. For some speakers the process may even become a perceptual constraint, so that they "hear" [nujen] rather than [nujen] (Stampe 1973b, page 35). We will return to the viability of such an account of loanword analysis, first in Section 6.3 and then more thoroughly in Chapter Seven below, and supporting cases are also available from Ohso's (1971) studies of Japanese.

The Weak Hypothesis relative to paradigmatic processes -- that such processes are a notion for formulating statements about the phone inventory of a Language L but do not result (at least for the adult grammar) in actual substitutions attested at the surface level -- seems to be the interpretation of Hooper (1975) in assessing Stampe's claims. While she defines Stampe's notion of paradigmatic processes as "a context-free rule that applies to underlying representation, and serves to enumerate the phonemes occurring in a language (1975, page 546), Hooper at the same time rejects the proposed analysis of the Vietnamese Nguyen example, offering instead the notion that the speaker may have in his inventory of native rules a process which is context sensitive, such that

(23) [+nasal] → [+anterior] / $ ___

to describe the surface distribution of [ŋ] (the alveolar nasal being

mandatory in syllable-initial position) and to account for loanwords without barring this segment from underlying representations. We might observe also that Lovins (1973, page 26) defines the notion of the paradigmatic process in not altogether different terms: "Eventually he [the child in the act of acquiring a language -- PCB] obtains some version of the phonology of L: a complex and often collectively cross-purposeful system of processes, some of them ordered, which are 'real' in that they correspond to actual substitutions in speech (those that merely delimit the phonological inventory need not apply in real-time as do lower-level processes of L, but would nevertheless be attested in actual speech in child language or in some Language L')" [with my emphasis -- PCB].

Hooper's proposal that a context-sensitive process of English governs the speaker's analysis of the loanword Nguyen should elicit some further comment. As we have seen, Stampe's account of how speakers might attack Nguyen also allows for analysis via a context-sensitive process as well as analysis by a paradigmatic process. But in Stampe's treatment the process involved is the well-attested process of regressive nasal assimilation, which allows the speaker to perceive Nguyen as parallel to engulf and thus results in a rendering like [ĩŋgujen] or perhaps [ɪ̃ŋujen] by backwards derivation (whereby the speaker starts with the surface form he perceives and runs backwards through the lower level processes, in reverse order, that must have applied in order for the utterance to have turned out that way). The existence of engulf and other parallels (forms with the structure $Vng) the speaker might draw demonstrate beyond doubt the existence of the nasal assimilation process of English; however, no alternations

of this type confirm the process Hooper proposes, leaving Stampe free to require a general ban on /ŋ/ in all positions underlyingly for English.

Of course, there is no more empirical evidence for the type process Stampe has in view than there is for Hooper's syllable-sensitive syntagmatic process. We would seem, then, to be left perhaps with an irresolvable case in light of which the best we can do is appeal to fiat and to the preferred analyses of one's adopted personal theory. At this point, let us ask instead the following two questions: (1) Is there any evidence that context-free processes could not apply in the approximate way Stampe suggests and that would therefore automatically rule out Stampe's analysis? and (2) If, in the absence of such evidence, we assume the possibility of such paradigmatic processes, is there any advantage which such an analysis would hold over other methods of arriving at what in actuality constitutes the deepest level of phonological representation (i.e. the lexicon)?

The attested existence of paradigmatic substitutions in child language and the less frequent but nonetheless attested applications of paradigmatic historical processes (those applicable in forming vowel inventories or in the origin and development of certain diphthongs in the English Vowel Shift) should suggest a sufficiently negative answer to the first question.

To answer the second question just posed, we must first recall that we have no direct evidence <u>of any type</u> revealing the exact nature of underlying forms in a phonology; we therefore must resolve that in the present state of linguistics and psychology these must remain accessible only through the rules or processes to which they are ex-

posed. Our evidence about proposed phonological forms at any level in the grammar remains exclusively our evidence about the rules. It is possible theoretically to set up elaborate and ingenious restrictions on underlying representation, and a number of schemes have been forthcoming in generative phonology (e.g. the Alternation Condition set forth in Kiparsky 1968a). Yet if we do not hypothesize that such restrictions arise from the operation of actual substitution processes attested at least at some point in grammars, then we are inevitably left with unproveable and therefore ultimately unworkable hypotheses. It is useful to elaborate with a particular case in point. We will restrict our discussion to the English velar nasal stop, not only because it is the segment at issue in Stampe's Nguyen example, but also because it is the focus of one of the best-known current debates involving an abstract representation in the present literature on generative phonology.

The surface distribution of the English velar nasal [n] is morpheme final position (as in words like king, tongue, sing(ing), and long(ing) etc.) and before the velar obstruents (as in words like hunger, tangle, angular, anxious, length, and linguistics). The difficulty quite obviously lies in the positing of underlying forms, and the two conceivable analyses have both had their proponents. The most widely accepted view is still that of SPE (pages 85, 211) which derives all instances of [ŋ] from /ng/ and might be deemed the abstract analysis: "In many dialects /g/ drops after nasals in word-final position but remains in word-medial position, so that we have [sIŋ] but [mIŋgl] (from underlying /sINg/, /mINgl/, respectively, /N/ being the archisegment 'nasal consonant'" (page 85). The opposing concrete analysis is that of Vennemann (1974a, pages 217-18) and is consistent with the

Strong Naturalness Condition of Natural Generative Phonology (cf. Vennemann 1971). This position demands that sing, singer, and finger would be similarly represented as /sIn/, /sIŋ+ṛ/, and /flŋgṛ/, while long, longer, and longest would have the underlying forms /lɔŋ/, /lɔŋ+ṛ/, and /lɔŋ+əst/, and the grammar would necessitate a g-Epenthesis rule to derive the surface forms [lɔ̃ŋgṛ] and [lɔ̃ŋgəst]. Vennemann's analysis predicts the eventual emergence of [lɔ̃ŋṛ] for the comparative form through the loss of the posited g-Epenthesis rule.

Another analysis, purportedly within the compass of Natural Phonology, is given in Rhodes 1974, where it is argued that certain natural processes like English Final g-Deletion are "boundary-conditioned" processes and operate only in the presence of word boundary. A corollary of this proposal -- in fact its central feature -- is the assumption that certain suffixes, such as in this case the English comparative -er, have the power to "degrade" the preceding boundary and assure that the "boundary-conditioned" process does not apply, while other suffixes, like the English agentive -er, do not possess such power. Within Rhodes's analysis, this "explains" the occurrence of longer [lɔ̃ŋgṛ] (comparative) with [g] and the occurrence of singer [sĨŋṛ] (agentive) without the phonetic [g].

Most linguists to date have supported some version of the abstract analysis. Even Malecót (1960), who provides some ingenius phonetic evidence for lack of a pronounced nasal segment in words like camp, hint, and bunk, admits the abstract nasal segment and non-nasal vowel and resulting assimilation by concluding that the nasality is that which "the vowel has acquired by anticipation of the nasal consonant" (page 228).

It is the concrete analysis, on the other hand, which if confirmed would effectively deny Stampe's claim for an active process ruling out unconditionally the velar nasal phone as an underlying segment of English. Verification of the analysis proposed by Vennemann, then, would establish that Stampe is simply wrong about the underlying inventory of English and about the role of at least one process delimiting that inventory. In this light, let us consider next a proposed set of hypothetical conditions on lexicalization which might provide some insights through which the concrete analysis of Vennemann would be either reasonably confirmed or summarily rejected as a descriptively accurate account of underlying segments in English.

Consider first the following condition, which is a further modification of Kiparsky's own adjustments of the original "segment paradigm condition" which was proposed in the form of the often-cited Alternation Condition governing the abstractness of phonology (Kiparsky 1968a).

CONDITION I ON LEXICALIZATION

The outputs of automatic, non-neutralizing substitutions never will appear in the lexicon.

Condition I is designed (in its original proposal by Kiparsky) to exempt from lexicalization by the "segment paradigm condition" invariant surface forms which can be attributed to automatic late phonetic rules. It rejects the extreme condition of the Strong Naturalness Condition which prompts Vennemann to include fine phonetic detail in his underlying representations (e.g. /t^{h}ɛləgraef/) and assert that "only allomorphs may be lexical forms" (1974b, page 347).

Since the absorption of the velar obstruent by the nasal in ng clusters (note this doesn't occur with contrasting voicing in nk) is a

natural process of English which is notably as automatic as is the aspiration on initial voiceless stops, there can be little objection that Condition I would be a motive for lexicalizing non-alternating surface forms like [sIn] and [dʌn] as /sIng/ and /dʌng/. But then Condition I must not stand alone, since it will provide as written for only the most superficial kinds of cases. Consider also:

CONDITION II ON LEXICALIZATION

Invariant outputs of all directly opaque, non-automatic, or legitimate neutralizing rules must be lexicalized.[22]

At this juncture we will introduce a parallel situation from Avestan (cr. Miller 1968) which appears to rule out reliance on Condition I alone and which fosters Condition II in its present form.

In the history of Avestan there is an attested change whereby the sequences of rt become [R] (for convenience here, this is some type of a voiceless r), this change provoking an initial assumption that this [R] might still be represented as /rt/, there being no autonomous /R/ and many examples of [r] derived from /r+t/. Yet when the substitution

(24) rt → R

was subsequently lost as a rule of the grammar, [rt] is found to resurface only where it has derived from the separate morphophonemes r and t (those cases where the requirement of the Alternation Condition -- i.e. "each segment appearing in the underlying form of a morpheme (excepting those resulting from automatic phonetic processes) must be present in that position in at least one allomorph of the morpheme on the surface"). [R] not alternating with [r] and [t] on the surface remains as an [R] that has apparently undergone relexical-

ization. But since rule (24) was automatic and non-neutralizing (there being no surface contrast between R and rt) and yet still the output was lexicalized, Condition I as a condition governing lexicalization has not been met. But Condition II would seem to provide the obvious and necessary solution. When we return to the history of Avestan, there appear to have been two distinct rules:

(25) r → R / ___ t

and

(26) t → Ø / R ___

(Miller 1968, page 282, provides the evidence in the form of several concrete examples.)

The point here is that the second rule (26) of t-Deletion, when it enters Avestan, has made an earlier r-Devoicing rule opaque (viz. Kiparsky's Second Condition on opacity whereby A → B / C ___ D is rendered opaque if there are extant cases of B in environments other than CBD) and it is this opacity, as Condition II captures it, which is motivation enough for relexicalization.

A current relevance of the Avestan situation seems to be, then, the degree to which the morpheme-final velar nasal in English provides an exactly parallel case and is therefore settled as well by Condition II on lexicalization. Just as t-Deletion rendered r-Devoicing opaque for Avestan, resulting in [R] being lexicalized in all non-alternating forms through Condition II, here (in English) g-Deletion (absorption of g by the preceding nasal) renders nasal assimilation opaque (for we now would have [ŋ] in an environment other than ng) and necessitates the parallel lexicalization of [ŋ] as /ŋ/. We have now, seemingly, provided a further motivation for the concrete solution of Vennemann,

beyond the strongest and most objectionable version of the "segment paradigm condition".

But we must remain more cautious of equating the two cases. I would suggest, in fact, that a closer examination in light of Condition II might seem to argue for lexicalization, instead, of /ng/, and not at all for /ŋ/. Perhaps even worse, the two conditions provided for resolving underlying representation may prove to be hopelessly contradictory in this particular case of the English segment [ŋ].

For whereas with Avestan we were concerned with the lexicalization of [R] exclusively (the output of the process which has been rendered opaque), with English we are concerned with the presence in the lexicon of not only /ŋ/ but also /g/. With Avestan there appears to be no real issue of underlying forms of the type /...Rt.../. Yet in English, for alternating forms like long/longer (comparative) the identical morpheme would apparently have to be represented in the lexicon in two distinct shapes (/lɔŋ/ for long and either /lɔng+r/ or /lɔŋg+r/ for the comparative form), an uncomfortable situation at best. The only clear solution is to appeal to Vennemann's g-Insertion rule for deriving longer, a rule which we should note has no more substantive support than Stampe's paradigmatic process (19). If we follow this line of reasoning and speculate that the above conditions on lexical representation dictate the zero realization of original morpheme-final g in forms like sing, long, and even longer once g-Deletion has become automatic, we are also faced at this point with the troublesome interpretation that this zero phoneme would result from a perfectly transparent rule of g-Deletion (applying even in the case of longer where g is re-inserted by g-Epenthesis) following a perfectly auto-

matic rule of nasal assimilation. By this reasoning, the zero phoneme (output of an automatic non-neutralizing substitution) should be barred from the lexicon by Condition I. And to make matters worse, nasalization would seem to be rendered opaque (the only excuse for lexicalizing the velar nasal under Condition II) by Vennemann's g-Epenthesis rule and not by g-Deletion. In short, the two cases do not seem quite the same, with the English case demanding a much more complex analysis.

But the case may be rendered even more hopeless still. Observe again that the outputs of r-Devoicing in Avestan and nasal assimilation before the morpheme-final g in English meet the proposed requirements for lexicalization strictly because of a condition of opacity. Both rules are automatic and non-neutralizing. But as we have noted, the opacity in these particular cases is opacity as defined by Kiparsky's Second Condition (viz. B in environments other than CBD, when these is a rule A → B / C ___ D). But it is precisely this notion of opacity which has recently raised serious and eloquent objections from Jonathan Kaye (1974b), who sufficiently demonstrates that rules made opaque by this condition are not necessarily in any sense the less natural or expected ones. And as we have also observed (see note seventeen for this chapter), it is this particular conception of opacity which appears to be altogether irrelevant as a condition on natural processes of the context-sensitive type (i.e. English nasal assimilation or g-Absorption), since these processes constantly will constantly create environments dictating that new and related processes apply.[23] In brief, Condition II hardly appears to be a workable and utilitarian condition applying universally to grammars.

What I am suggesting here is that arbitrary conditions proposing

formal restrictions on lexical structure of the type we have examined seem to offer little hope for resolving the issue of abstract representation in phonology. The only impeccable evidence for the shape of the speaker's underlying forms is the indirect evidence of attestable phonological processes. The conditions on lexicalization proposed above do not seem to obviate the possibility that English speakers might reject /ŋ/ as a possible underlying segment (and thus they do not damage Stampe's proposal that this apparent gap in English lexical structure results from an actual substitution as in (19) above), just as Chomsky and Halle have not produced any proper evidence against the verifiability of the phoneme. At this point, at least, we are not intending to argue that the Natural Phonology solution is entirely the correct one either, but only that it has not yet been refuted. For the claim that English [ŋ] is derived and never basic there is even a moderate amount of empirical evidence of at least a tentative sort. Lester and Skousen (1974, page 237) offer examples from the analysis of drunken speech based on spectrographic evidence that sing will be pronounced [sĩnk] as well as hand being pronounced [hæ̃nt] and bed [bɛt]. The pronunciation of sing cited here would demonstrate the psychological reality for these speakers of underlying /g/ since other reported examples by Lester and Skousen of drunken speech provide no evidence of the voiceless stop being pronounced at the end of a psychologically real word-final nasal (there being no pronunciations like [pĩnt] for pin or [mãmp] for mom). And slips-of-the-tongue provided by Fromkin (1971, page 34 and following) -- pronunciations like [slg...mæ̃n] for sing...man, [tšʌ̃nk yʌg] for Chuck Young, or perhaps [kʌ̃nt...strIg] for cut...string -- seem to provide a type of confirming

evidence. Of course, we could also conclude that even supposing that English did turn out to have a verifiable /ŋ/ in the lexicon (and the same might be argued about, say, nasal vowels) this would still decide nothing conclusively about Stampe's notion of paradigmatic natural processes. Present-day English might simply have lost such a process which it earlier had possessed.

To repeat the arguments of this section more succinctly, I have proposed that exclusively theoretical arguments of the type represented by Halle's rejection of the phoneme or by arbitrary conditions on lexicalization do little to rule out basic claims of Natural Phonology. Furthermore, I have been suggesting that Stampe's proposals for a natural phonemic level and for dominant paradigmatic processes provide what amounts to a preferred theoretical position -- one both explanatory and more susceptible to experimental verifications -- since the first porposal (the natural phonemic level) will rescue the validity of well-entrenched phonemic constraints like biuniqueness and invariance while the second (paradigmatic processes) replace hopelessly arbitrary conditions on lexicalization with process-type substitutions which offer at least some moderate degree of empirical support.

NOTES

[1]A preliminary version of some of the text of Section 2.1 and Section 2.2 were first presented in my paper delivered at the Summer Meeting of the Linguistic Society of America in July 1974 and cited here as Bjarkman 1974a. Also, an initial and somewhat different written draft of these same sections has already been published as Bjarkman 1975. It should be noted, however, that my position on a number of issues (most especially the analysis of Greek r-aspiration treated in this chapter and the question of allophonic processes taken up in Chapter Three and again in Chapter Seven) has been greatly altered since those earliest versions. I am especially grateful to Gary Miller for his discussions and criticisms of several revisions of this part of the dissertation, as well as for supplying originally so many fine points for argumentation.

[2]Most papers in the 1974 C.L.S. Natural Phonology Parasession volume do not actually deal with Stampe's idea of Natural Phonology at all, though a few (e.g. those by Ohala, Lee and Howard, and Dressler) do attempt a direct rebuttal. Other treatments are still rare. Julie Lovins has reported to me (personal communication) that one manifestation of an interest in Stampe's theories will be a forthcoming work by M. Nishimitsu aiming at "an overall analysis of Japanese phonology in the framework of Natural Phonology based on its historical change and children's phonological acquisition and dialectal variation" (this being Lovins's direct quotation of Nishimitsu).

[3]The most significant work along these lines to date seems to be the paper by Richard Wojcik on morphological conditions governing the application of processes (although see note six in Chapter Three).

[4]Rhodes emphasizes that the "level" Stampe has in view is not in any sense equivalent to the Classical Phonemic level appropriated from American Structural linguistics. Actually, Rhodes is responsible for the label "natural phonemic level" (it appears first in Rhodes 1973) -- Stampe himself having eschewed all references to "levels" and maintained only the designation of "phonemic representation."

[5]The reader is encouraged to see Stampe's own discussion of the mentalistic motivation of processes in Chapter One of Stampe 1973a, as there is no space for elaborate treatment in this present chapter.

[6]We must leave aside here the fascinating question of what the child's actual underlying form is for each of these words and role the rules of perception might play. Does the child have /dɔg/ and then apply Lowering? Or does he lexicalize /dag/?

[7]My own daughter first had both alveolar and bilabial nasals, initially and finally (e.g. Nikki, Nickey Nouse, gome (for "gone"), nite nite, no, etc.), then she applied a context-free process converting all nasals to labials (Mikki, Mickey Mouse, gome, mite mite, mo, etc.), which was eventually suppressed in light of the adult standard. This sudden appearance in her speech of only labial nasals supplements numerous similar examples in the work of Stampe (child language) and Patricia Donegan Miller (universal vowel inventories) of the reality of such paradigmatic processes. It would seem obvious that my daughter had underlying n in words like Nikki (the name of a playmate), nite nite, and no before this process appeared in her speech.

[8]This issue of whether rules (i.e. substitutions of all types) are sequential is currently a subject of considerable debate and it would seem better to talk of "applications" of rules, rather than the rules themselves, as being sequentially ordered. My position on the structure of a phonological component, including the notion of sequentiality, is made more precise in Section 3.1.2 (where I define Stampe's notion of "phonemic" level, as I find it to be implicit in his definitions of "allophonic" and "morphophonemic") and Section 4.2.0 (where I present my own tentative model of Natural Phonology). For a strong statement against the position that a phonological component can be divided into blocks or components of rules, see the arguments provided in Anderson 1974, 1975.

[9]Anderson exemplifies this argument at its fullest in these terms:

> In general, the application of a phonological rule does not affect the transparency of a morphological environment, but the presence of a particular affix, ablaut vowel, reduplication syllable, etc. can obviously affect the environment for a phonological rule. Thus, in general, the phonological rule will be more transparent if it applies before, while the morphological rule will be equally transparent regardless of its ordering. Ceteris paribus, then, the expected ordering for such a pair will place the morpholexical rule first. It is exactly where the transparency of the morpholexical rule is affected by the phonological rule that the reversed ordering can appear: thus, reduplication in Tagalog, Cebuano, Luiseño, etc. is more transparent if its effects are not obscured by other rules; derivation of imperatives from infinitives in Danish is more transparent if other processes differentiating infinitives from stems are allowed to apply first; and the loss of /y/ is in Abkhaz more transparent if it does not entail alternation in the placement of syllabicity and stress (1974, page 7).

[10]The fact that this example is not entirely undisputable is to be taken up below in this chapter.

[11]These are the standard and often repeated examples. Others for English can be found in Stampe 1973a, 1973b and in Fromkin 1971. A few examples of these relevant words in English would be fungi, pedogogy, and opacity (but note e.g. lickity split).

[12]The question of the sequentiality and linearity of rules and processes is most insightfully discussed, as already mentioned above, in Anderson 1974, 1975. See also Hutchinson 1973, taken up immediately below.

[13]It should not go unnoticed here that not only do Lee and Howard misinterpret Stampe throughout, but also they crucially misquote him. Notice that their quotation given on page 222, for example, as

> "[t] could have flapped optionally at any time, but it does not delete until it becomes syllable-initial at 9" (p. 57)

should in fact read

> [t] could have flapped optionally at any time, but it is not deletable until it becomes syllable-final at 9..... (p. 60)

[14] Of course for Stampe the unpronounceable stages in any derivation are those to which an obligatory process has not yet applied. They are never stages which would violate Postal's Naturalness Condition. That is, there are no abstract segments; even unpronounceable forms are entirely in terms of pronounceable segments.

[15]The crucial point here, certainly, is that unlike the cases invovling the Syllabication and Nasalization processes which Lee and Howard discuss earlier, it seems quite a bit less obvious here that two distinct processes might be at issue.

[16]These two derivations, taken side by side, create a special problem with regards to the ordering of allophonic and morphophonemic processes (that is, in accordance with Stampe's definition). In the righthand derivation, the fact that voicing of č to ǰ (replacing an underlying segment with a purely allophonic one) precedes and actually establishes the environment for the voicing of š to ž (both underlying segments of Russian) would seem to provide us with a case of allophonic processes preceding a morphophonemic one. This would not only contradict the model of Natural Phonology for which I will argue in Section 3.1.2 and Section 4.2.0, but also effectively obviate Hutchinson's (1973, page 67) strong constraint ("The Principle of Morphophonemic Precedence") on sequential applications:

> In all natural language phonologies, all rule applications with morphophonemic effects must be made before rule applications with allophonic effects.

This is a seemingly unique case, however, where application of processes with allophonic effects will establish an environment for processes with obviously morphophonemic effects, and I know of no other examples of precisely this kind. One potential example from Danish where a morphophonemic process (nasal assimilation) follows an allophonic process (a vowel quality rule specifying the degrees of frontness) is cited in Anderson 1975, page 54, yet in this case I take the status of velar nasals as underlying segments (a basic assumption for nasal

assimilation to be viewed a morphophonemic process) to be somewhat suspect. We may in the long run only be able to appeal here to some general principle which says that, given a particular string as input, all available morphophonemic processes that could apply to that string would come first. If new environments happen to be created for morphophonemic processes, they would have the same propensity to reapply that characterizes all processes. This question will have to be broached somewhat further in Section 4.2. Also see note nineteen below.

[17] Anderson, in the articles already cited, extends the argument against linearity to all types of phonological "rules". The suggestion made here is simply that linear order is not relevant with processes. The question involving the learned "rules" must be left open, though argument by Anderson and others against extrinsic linear ordering for this type seem also persuasive. Anderson is convincing, in particular, that ordering of rules is tied in with a concept of rule opacity. Though the general principle of ordering is morpholexical/phonological/phonetic, the morpholexical rules follow rather than precede certain phonological rules predictably in cases where this deceases rule opacity. It is worth mentioning at least at this point, then, a point to be developed in a later section of this chapter. This is that rule opacity in the second sense defined by Kiparsky (an occurrence of B in environments other than C___D following application of the process A → B / C___D) is clearly irrelevant to natural processes of the context-sensitive type, since such processes constantly create environments which demand that new processes apply. Nasal Assimilation before final /g/ in English is regularly followed by Final g-Deletion, so that we have B in environments other than C___D on the surface. This is a phenomenon which is highly encouraged and not discouraged by natural languages.

[18] The constraint on rule application as Hutchinson presents it is quoted in note sixteen. Again, Hutchinson is specifying phonological rules in the SPE sense of "rules" and does not acknowledge the "rule/process" type dichotomy made by Stampe. The purpose of Hutchinson's article is avowedly to support the intrinsic ordering hypotheses popularized by Koutsoudas (the KSN Hypothesis) and his principle is identical to the earlier-mentioned "Principle of Morphophonemic Precedence" also presented in Koutsoudas and Dinnsen 1976.

[19] Rhodes (personal communication) objects that examples of such neutralizing processes following in the ordering processes that never will neutralize are abundant in natural languages and make possible the strong assertion that all allophonic processes are preceded by all morphophonemic processes (see Chapter Seven). The first example he cites as illustration is from English where the process

ən → n̩ / [+obstruent / +apical] ___

bleeds Flapping

*(t, d) → ɾ / V ___ V̆

*e.g. [smítn̩] "smitten"

[sɛ̃́nʔn̩s] "sentence"

etc.

Yet such an example is irrelevant to the issue as both processes are indisputably allophonic (introduce segments that are not part of the underlying inventory of English). I can not comment with the same confidence about his numerous examples from Sayula Popoluca (personal communication), though I suspect the same confusion of process types may be the case here as well. However for a modification of my view of the notion of allophonic versus morphophonemic see Chapter Seven.

On the question of neutralization, the example of writer and rider which has become such a regular feature of generative phonological discussions also merits our attention. Here a phonological process which is a phonetic detail rule specifying degrees of vowel length precedes the process of Flapping which both manipulates features which are distinctive at the underlying level and neutralizes an underlying distinction between /t/ and /d/. But notice that the segment introduced ([D]) is not underlying in English and the process is thus by definition allophonic. The significance of this particular example is its illustration that neutralization by itself is not a sufficient definition of morphophonemic.

[20]The example here is due to Wojcik (1976a, page 2). The lexical and systematic phonemic levels are taken here, as in SPE, as differing only to the extent that the latter has fully specified matrices while the former does not. Notice that the process here is Progressive Voicing Assimilation (which in this case results in the devoicing of z after p) and not Final Devoicing. English has no such process of Final Devoicing.

[21]As Hutchinson puts it

> But surely this is a non-sequitor of classical propositions! Russian phonology has not been rendered more complex by requiring phonemic representations, Halle-style phonology has been rendered more complex. Certainly we are free to argue that it is therefore Halle's theory of phonology that is to be rejected, not phonemic representations. In fact, we could claim that any theory which cannot incorporate such a level and still make desirable generalizations is ipso facto descriptively inadequate. Given that the phonemic level of representation was an acceptable part of phonological theory for at least four decades in this country, that its worth as a theoretical construct was universally recognized, it is almost incredible that Halle's argument was not taken as a crushing indictment of generative phonology (1973, page 64).

[22]This condition was originally suggested to me by Gary Miller. Miller has since, however, disavowed any support for such a formulation as a workable universal condition on grammars.

[23]The case of Rumanian palatalization, discussed in Schane 1971, page 505, is another exceptionally good example of the evidence that Case Two opacity as proposed by Kiparsky is altogether irrelevant to natural processes as opposed to learned phonological rules. Of course, this is only to be expected when we recall that Kiparsky's original notion of opacity had to do with some sort of evaluation of the

"learnability" of a rule -- a condition which is by definition irrelevant to processes, which themselves originate as innate restrictions on pronounceability.

CHAPTER THREE

COMPETING MODELS IN NATURAL PHONOLOGY

> Among the various theories of Natural (Generative) Phonology the one of Stampe (1969) is outstanding, at least because of the early time of its conception and the range of its postulated explanatory power.....only Stampe's Natural Phonology combines intimately phonetic theory, language typology, loan word phonology, the study of casual speech with child language acquisition and historical change.
>
> -- Wolfgang Dressler

3.1 Explanation in Stampean Natural Phonology

Natural Phonology as Stampe conceives it follows from a Phonemic Hypothesis (viz. grammars contain three significant levels of formal representation: phonetic, phonemic, and lexical), as opposed to the Systematic Phonemic Hypothesis advanced in SPE (Chomsky and Halle 1968, where only two levels of representation are significant: the phonetic and the systematic phonemic which is underlying).[1] Few if any phonologists seriously advocate a Phonetic Hypothesis (i.e. that only phonetic representation is relevant to grammatical analysis).

In the Natural Phonology analysis of language acquisition, the child is assumed to adopt an initial underlying phonological system which approximates the phonemic system of adults and to then postulate more abstract forms only when these are needed to account for more troublesome phonological alternations. Such troublesome alternations might be e.g. like those between the German singular /bunt/ "association" and plural /bunde/ "associations" (compare /bunte/ ~ /bunte/ "colorful") or Spanish [kontar] "to count" and

[kwénto] "I count" (on Spanish Diphthongization see Saciuk 1969, Section 4.1.2). By "adult phonemic form" we mean here something approximating what Chomsky labels a set of phonetic detail rules. Forms which are without alternation will retain shallow phonemic representations and even in the adult grammar underlying forms correspond to a traditional phonemic level. Only the truly allophonic features (in Stampe's terms those features previously eliminated by context-free processes filtering the lexicon) are barred from underlying representation. Support for this Phonemic Hypothesis can be drawn from examples in linguistic change and in child language which suggest speakers universally assume the unmarked value for any phonetically neutralized feature (cf. Stampe 1968).

Whereas Chapter One served as a general introduction, Chapter Two was devoted to misinterpretations in the literature deriving from Stampe's notion of phonemic level and the distinction rule/process in Natural Phonology. This section outlines some additional hypotheses which are the cornerstones of Natural Phonology and summarizes some supporting arguments which have appeared for the most part in Stampe's unpublished works (Stampe 1968, 1973a, 1973b) and are likely unfamiliar to the majority of phonologists. It is my central thesis that Natural Phonology is not reducible to a notational variant of SPE -- one which might have a simple analogue in the division of the standard generative model into "phonological" rules and "phonetic detail" rules -- but instead constitutes a radically different approach to "explanation" in phonology which offers solutions totally unavailable within the standard theory. In brief, this chapter makes some small progress toward establishing the distinction of natural versus generative as

more properly the distinction of "explanatory" versus "descriptive" in phonological theory.[2]

I will first explore the problems of abstractness and ordering and the motivations for phonetic change as these are uniquely handled within Natural Phonology (Section 3.1.1). A first feature of Stampean phonology to be emphasized is an assumption that any system of phonological processes exhibits real substitutions found occurring in the mental and physiological performance of an actual speech act and that all underlying as well as superficial representations ("surface forms") of these utterances have a parallel reality for the speaker. Stampe's theory, then, is a strong rebuttal to what Kiparsky characterizes as the completely abstract approach to morphophonemics (Kiparsky 1968a), and a considerable variation on the theme of process morphophonemics as well.

A second feature is an assumption that phonetic change occurs not through simplification in the adult grammar (compare e.g. versions of the standard theory as advanced by Kiparsky 1965, 1968b or Dinnsen 1974) but instead through the child's "imperfect imitations of adult speech" (Stampe 1969). I will emphasize in the middle portions of this section (3.1.1) Stampe's contention that rule addition in phonetic change is alternatively to be explained as a failure by children to suppress some relevant natural process of their language and that apparent "generalization" of rules is likewise a failure to limit processes of this innate system.[3] The evidence I present here will be largely Stampe's own examples, since it is my intention in these preliminary chapters to explain rather than modify or advance in any measurable way the theory of Natural Phonology. Corroborative

evidence from my own research into the speech patterns of Miami Cuban informants is reserved for later chapters.

In Section 3.1.2 my purpose is to clarify the concept of phoneme in Natural Phonology and to provide some of the kinds of evidence adduced. The conception of the phoneme integral to Stampe's analysis of English or to my own analyses of Cuban Spanish (Chapter Five and Chapter Seven) seems to fall closest to that of classical phonemic theory (or pre-generative phonemic theory) and performance data suggests such representations do have an actual psychological reality for the speaker: i.e. phonemic representation is that intermediate stage more abstract than phonetic forms which exists for speakers in cases where processes of neutralization have applied. One type of evidence for such representations is what happens in dialects where these morpho-phonemic processes are lost (viz. Kiparsky's frequently-cited Swiss German example discussed below). A second type seeming to argue just as persuasively for the Phonemic Hypothesis is Shibatani's observation that speakers will reject as proper forms cases where processes like Final Devoicing have failed to apply (Shibatani 1973, page 95). What seems suggested by such instances is that speakers maintain somehow two levels of abstract representation: the level at which forms are stored in a lexicon and the level of an "idealized" representation, the latter being the level presumably at which one maintains phonological representations when processes of neutralization apply and the level at which what have been called surface phonetic constraints abound.

In brief, Section 3.1 is a review of the distinction "rule/process" as well as a discussion of five defining hypotheses underscoring the direction of Natural Phonology. Emphasis here will be on empirical

evidence which supports phonemics and solidifies a concept of processes as well. In a concluding note (Section 3.1.3) I will take up one further lingering problem from Spanish phonology in light of insights provided by these notions of Natural Phonology. Cressey (see Cressey 1974) has argued that problems earlier encountered by Harris (1969) in attempting to resolve the controversial issue of underlying forms for some Spanish glides are potentially resolved by making an assumption that there must exist at least two similar glide-formation "rules" -- one a "marking convention" and the other a late variable rule of Spanish. A more natural explanation would seem to be that the two substitutions here involved are (1) a proper phonological rule appealing to the notion of word boundaries, and (2) a context-sensitive natural process of vowel strengthening. The historical development Cressey has in mind likely was from the process to a learned rule and not the highly implausible conversion of language-particular rule to universal marking convention (especially since such conventions may well have no substantive reality in psychologically-real grammars at any rate). The Spanish glide-formation problem, then, stands as an excellent illustration of explanatory powers of Natural Phonology over generative phonological theory. To support this case still further, a parallel illustrative example will also become available once we make a similar reanalysis of Daniels's unsatisfying and often unexplanatory study of the Russian consonant assimilations in Daniels 1973.

3.1.1 Hypotheses in Natural Phonology

Assumptions behind the strong claims of Natural Phonology may be reduced conveniently to a series of empirically-testable hypotheses

about language acquisition (i.e child phonology) and language change (which Stampe also contends is intimately bound up with issues of child phonology). I will begin by summarizing as briefly as possible a list of these, which I will refer to (using labels which are my own and not Stampe's) as the Innateness Hypothesis, the Suppression Hypothesis, the Phonemic Hypothesis, the Language Acquisition Hypothesis, and the Phonetic Change Hypothesis. Taken together, such hypotheses form the very core and substance of Stampe's phonological theory.

3.1.1.1 The Innateness Hypothesis

"The phonological system of a language is largely the residue of an innate system of phonological processes, revised in certain ways by linguistic experience" (Stampe 1969, page 443). This is the most crucial assumption made by Stampe and one from which all else in Natural Phonology will more or less eventually follow. In view of the standard notion of linguistic rules the claim is especially radical: much of what has been traditionally called the "phonological rules" in the process approach to morphophonemics are in actuality the innate restrictions on human speech capacity which are better viewed as the natural processes of the phonology. That these processes are different from learned rules in concrete and definable ways is demonstratable (see Chapter Two), and the bulk of our evidence from child language is a seemingly irrefutable demonstration that process-type constraints are innate and govern phonetic behavior from the very onset of speech (see Edwards 1970, as well as Stampe 1973a, 1973c).

Processes work to merge numerous phonological oppositions into those segments of these oppositions which least strain the governing restrictions on physical speech systems. Quoting again: "A phonological

process is a mental operation that applies in speech to substitute for a class of sounds or sound sequences presenting a specific common difficulty to the speech capacity of the individual, an alternative class identical but lacking the difficult property" (Stampe 1973c, page 1). Such processes occur in context-free and context-specific varieties, and the list given in Figure One will serve as examples of some commonly observed processes of English.

(1) FIGURE SIX

SOME CONTEXT-FREE NATURAL PROCESSES OF ENGLISH[4]

VOWEL DENASALIZATION	$\tilde{V} \rightarrow V$	
GLOTTAL STOP DELETION	$? \rightarrow \emptyset$	
h-DELETION	$h \rightarrow \emptyset$	(except / $ _ (y) V)
NASAL ELIMINATION	$N \rightarrow [n]$	(except / $[\ \underline{\quad}\ _{+labial}\ ]$)
GEMINATE CONSONANT REDUCTION	$CC \rightarrow C$	
GEMINATE VOWEL REDUCTION	$VV \rightarrow V$	

SOME CONTEXT-SPECIFIC NATURAL PROCESSES OF ENGLISH[4]

BG DELETION	$\begin{bmatrix} +stop \\ +voice \\ @position \end{bmatrix} \rightarrow [\ +nasal\ @position\ ] \ /\ \_\ \$$ (except / $[\ \underline{\quad}\ _{+alveolar}\ ]$)
PALATALIZATION	$\begin{bmatrix} +obstruent \\ +alveolar \end{bmatrix} \rightarrow [\ +palatal\ ] \ /\ \_\ y$
VOWEL NASALIZATION	$V \rightarrow \tilde{V} \ /\ \_\ [\ +nasal\ ]$
FLAPPING	$\begin{bmatrix} +stop \\ +alveolar \end{bmatrix} \rightarrow D \ /\ V(c)\ \_\ V$
GLOTTAL STOP INSERTION	$\begin{bmatrix} +stop \\ -voice \end{bmatrix} \rightarrow ? \ /\ \_\ N$
NASAL ASSIMILATION	$n \rightarrow [\ @position\ ] \ /\ \_\ \begin{bmatrix} +obstruent \\ @position \end{bmatrix}$
GLOTTAL STOP INTRUSION	$\emptyset \rightarrow ? \ /\ \$\ \_\ V$

One corollary of an Innateness Hypothesis of language acquisition is that such natural processes provide the available phonemic inventory. The best explanation is found in the introductory portion of Donegan Miller's thesis (1972a, page 136) on vowel inventories:

> According to Stampe's view, a process affects a class of segments which share a feature that is inaccessible to the inborn capacity for speech. For these segments, the speaker substitutes segments from another class identical to the first except that the inaccessible feature is eliminated. In general, then, segments with fewer inaccessible features are substituted for those with more -- in regular fashion. Thus, the first segments acquired by children will be those with fewest "unusual" or inaccessible features. In order for any but the simplest segments to be acquired, the speaker must suppress or limit the processes which simplify the more complex segments. In view of this, the phonological inventory of a language may be described in terms of the suppressions that the language requires of its speakers. For instance, if a language admits a y vowel, it has suppressed the natural process which unrounds palatal vowels -- i.e., which substitutes i for y (my italics -- PCB).

It is also a corollary of an Innateness Hypothesis that processes come in contradictory sets reflecting conflicting phonetic restrictions. Vowels -- to give one familiar example from the brief list of English processes above -- are subject in English as in most languages to an unrestricted denasalization process, which accounts e.g. for the English treatment of many French loans. Yet a context-sensitive nasalization process which is a form of assimilation introduces surface nasal vowels before other adjacent nasal segments. Similarly, obstruents are most naturally voiceless, irrespective of context, simply because oral constriction involved in their production greatly reduces the vocal airflow required by glottal vibrations. Nevertheless, the voicing environments between vowels or vowels and other syllabics will favor the voicing of obstruents by assimilation. In either case, we discover a tension between a dominating process which governs underlying inventory and a contextual process which facilitates articulations.

Since these processes are assumed to make real substitutions of actual pronounceable segments, when such competing processes emerge -- as when ae have normally voiceless obstruents in any voicing environment or vowels adjacent to nasals -- some specific mechanisms for resolving this incompatability must be sought out in the grammar.

3.1.1.2 The Suppression Hypothesis

Natural Phonology provides for competing processes being potentially resolved in just these circumstances in one of three possible ways:

1) A suppression of one of the contradictory processes

The speaker may simply elect to overcome the devoicing process operating on obstruents, which as we have suggested is a natural but not obligatory substitution in his grammar. Or he may suspend either processes of nasalization or denasalization. Examples of such suppressions which seem necessary to account for the vowel systems of individual languages abound especially in studies by Donegan Miller (1972a etc.): (1) Languages like Bororo and Maidu which reveal three-timbre-class systems (viz. [+Pal, -Rnd], [-Pal, -Rnd], and [-Pal, +Rnd]) arrive at those particular systems when the Neutralization, Lowering, and Raising processe are suppressed but Depalatalization and Unrounding still affect the low vowels (yielding the vowel inventory i,e,ɨ,ʌ,a,u,o); (2) A simpler two-height system like that of Amahuaca occurs when only Neutralization has been suppressed and Raising and Lowering are now allowed to operate (yielding the vowel inventory i,ɨ,a,u); (3) But when Depalatalization and Unrounding are also suppressed, then the nine vowel system of Trukese and Thai results (yielding the inventory i,e,ae,ɨ,ʌ,a,u,o,ɔ).[5]

2) A suppression of some part of a process

A less radical type solution, this may be the limitation of the

set of segments to which a process might apply, or of the set of contexts which is relevant. But any such limitations will follow the strictest hierarchies of applicability. Thus devoicing may be limited to just tense obstruents unfavorable to voicing, but never will it be restricted to the more readily voiced lax ones.

An example emphasized by Donegan Miller (1972a, page 1957) is that of Joan Velten's (Velten 1943) first distinction, made at fourteen months while formulating her infant vowel system, between high u and low a. Joan could apparently only have accomplished this distinction through a limiting of the Neutralization process from Process A (especially unstressed and lax vowels become [-High, -Low, -Pal, -Rnd] to Process A' (especially unstressed and lax vowels become [-Pal]). No other solution so enticing seems immediately to present itself.

3) A resolution by ordering of processes

If a general devoicing process brings about an exceptionless loss of the voicing opposition in obstruents, however, a later context-sensitive voicing process functions to restore voiced obstruents in just the expected voicing environments, then it can be deduced that any application of these processes in an opposite order would be quite indistinguishable from an actual suppression of the later voicing process.

Here, as with nearly all ordering phenomena in linguistic theory, it is possible to elicit only somewhat indirect examples for resolutions of conflicting tendencies through a reordering of processes. To do so, we need only recall the first assumption of Natural Phonology: conditions on underlying representation have their origins as innate natural processes which, in the child's speech at least, cause actual substitutions at the surface level. We might then assume with Stampe that when these processes

disappear (meaning only that their effects are no longer apparent) they have not necessarily always been suppressed, but instead sometimes merely ordered upwards where they yet continue to play a role as restrictions, though now relative to underlying rather than surface representations. Stampe offers as evidence for reordering the following deletion phenomenon of American English:

> For example, children's pronunciation of English often exhibits an innate process deleting glottal stops, so that [bʌʔn̩] "button" becomes [bʌn̩]. When these deleted glottals at last appear in the child's speech we might assume that the child has suppressed the deletion process. In fact it appears that he has merely ordered it to the north of the process which substitutes glottal for [t], and that it is this deletion process (not an unrelated, coincidental condition) which accounts for the inadmissibility and unpronounceability of nonderived glottal stops in English. It is no accident, therefore, that adult relaxed speech commonly exhibits deletion (via unordering) of derived glottal stops. Nor is it an accident that in foreign words with glottal stops which cannot be analyzed in the English system as derived from [t] are deleted (1973c, page 48).

Such suppression mechanisms are crucial to a system of phonology of the type defended by Stampe. Each phonetic opposition the child will be required to learn on the lengthy road to adult speech patterns must involve some such revision of his innate phonological system. A child must work to revise those features of his speech which are still keeping his utterances distinct from the adult standard -- a standard he is rewarded constantly by fawning parents for emulating. And this revision is over and over again carried out by means of the same few mechanisms which are functional in resolving all phonological contradiction -- suppression, limitation, and the reordering of natural processes.

In its first primitive stages an innate phonological system should be assumed to be a full array of the restrictions governing pronounce-

ability. All the unordered and unlimited phonological processes are together and simultaneously available. The mature speech system of the adult has retained from this confusion of contradictory substitutions and limitations just those aspects of this innate system that have been left intact throughout this effort at mastering adult pronunciations. The processes which survive this "unnatural" limiting of the inherent tendencies toward mental and physiological ease of articulation will yield eventually the pronounceeable phonetic representations of the language. The Suppression Hypothesis dictates that a failure to limit processes in precisely the way being described would potentially reduce the child's grammar to identical utterances made up of a single lax stop and low vowel, which is the very point at which the infant has begun (and quite obviously all that could hope to result from a system permitting all processes to apply and wipe out anything but the articulatorily simplest consonant and the maximally vocalic vowel).

Stampe's observation is, then, that as a child acquiring the adult grammar you don't learn merely by "doing what comes naturally." Thus language acquisition for the child is quite evidently as much a matter of his limiting processes that are natural as it is a matter of his acquiring "rules" that are unnatural.

3.1.1.3 The Phonemic Hypothesis

The Phonemic Hypothesis, as we will define it at several points, contends that it is a more shallow level of phonemic representation and not the more abstract systematic phonemic level which is the basic or relevant level of phonological description. It is here that we distinguish which processes may (since they are ordered earlier) or may

not (since they are ordered later) constrain phonological representation as opposed to phonetic representation. It is this level which Wojcik in a recent paper (Wojcik 1976) attempts to clarify with the more descriptive label "concrete phonemic" level.[6]

This Phonemic Hypothesis finds its most dramatic support in certain diachronic examples, such as a case popularized especially by Kiparsky where Final Devoicing is lost in Yiddish dialects (Kiparsky 1968a, 1968b; Stampe 1968; Bjarkman 1974b). It has been a popular subject in the literature to discuss how these dialects suppress a morphophonemic devoicing process prevalent in Germanic languages, with the resulting reappearance of final voiced obstruents. Loss of Devoicing results in forms like [vɛg^{h}] (with the Genitive [vɛgəs]) for what had at an earlier historical stage been [vɛk^{h}] (deriving from the underlying /veg/ "road").

What Stampe seizes on as being somewhat remarkable, at least once given the position on abstract representations espoused by the standard theory, is that the perfectly regular reflex of this loss for all speakers without exception is the final voiced obstruent and not the random pronunciations which might be expected if the phonetically neutralized feature "Voice" were left as unspecified in the deepest structure as Chomsky and Halle propose.

> [Under the Systematic Phonemic Hypothesis of Chomsky and Halle] no phonological processes which govern alternations (whether morphophonemic or allophonic) ever constrain lexical representation. The effect of this is to require that in a form whose alternants do not happen to reveal all the features of its phonological representation, these features are represented as unspecified in phonological representation. We are not concerned here with all the redundant features, but only those whose specification results from the application of proper phonological rules, i.e. of processes actually governing substitutions.....If the phonemic hypothesis is correct, under any circumstance which prevents a morphophonemic process from applying to a phonologically neutralized segment, it should regularly be pronounced as if it were

> phonemically represented, i.e. with the unmarked value of the feature. If the systematic phonemic hypothesis is correct, on the other hand, the unspecified nature of the phonologically neutralized feature should result in random pronunciations, some marked and some unmarked (Stampe 1968, pages 4-5).

Of course it might be claimed about the forms from Lithuanian Yiddish that in cases like original [vɛk^{h}] (as with German [b$\tilde{o}$t^{h}] and [b$\tilde{o}$də] "association(s)") the voicing in final obstruents was only phonetically neutralized. Presumably such alternations as [vɛk^{h}] with [vɛgəs] could be called upon by supporters of more standard approaches to diachronics to account for the speaker's ability to consistently recover voiced obstruents. The historical evidence which more firmly supports the phonemic hypothesis, then, seems to lie rather with the forms which did not altenrate (i.e. were phonologically neutralized) yet after the loss of Final Devoicing are realized only with voiceless obstruents. At the time of this loss, [ʔap^{h}] "from" and [ʔavɛk^{h}] "away" apparently must have been phonologically represented with voiceless obstruents, despite their forms in earlier stages of German. We will return to the subtleties of this last example below in Section 3.1.2. What is vital at this juncture is only an observation that for Yiddish speakers the Phonemic Hypothesis seems borne out: under circumstances preventing the application of a morphophonemic process to a phonologically neutralized segment (viz. obstruent Devoicing to final obstruents) the form is quite regulalry pronounced exactly as it might be phonemically represented. That is, in its unmarked form.[7]

3.1.1.4 The Language Acquisition Hypothesis

It is an old observation that children from all languages exhibit considerable regularity with the order in which they master adult forms, and although it has become increasingly more apparent in recent studies

of child language that different children will acquire sounds and patterns in remarkably different orders, the progress of acquisition as it is described by Stampe makes answers to this apparent ambiguity available as well.

It is demonstratable that most of the reported diversity in the child's acquisition of speech is only apparent. What appeared to early investigators to be radical and divergent substitutions (e.g. Joan Velten's replacement of [l] with [z] reported in Velten 1943) turns out to be a series of distinct minimal substitutions quite as common (though perhaps in some different order) as the substitutions made by countless other children (Stampe 1973a).

Compatible with this regularity is the claim in Natural Phonology that the child has a phonemic system parallel to that of his adult models. After exhaustive research (Kornfeld 1971 and Ingram 1974), there is still nothing that resembles acceptable evidence supporting a more traditional claim children have highly individual and diverse underlying systems which might rule out facile and regular adoption of standard adult pronunciations.[8] In contrast, striking evidence exists for Stampe's belief that the child has quite early adopted something corresponding to an adult phonemic system. This is seen when the child suddenly and irreversibly masters adult oppositions he has heretofore previously merged. In an example already cited, a widespread process of child phonology deleting final stops gives English dog initially as [da], yet after the acquisition of velars dog normally converts to [ga], with an assimilation apparent to a deleted velar which will not surface (e.g. in pronunciations like [gIŋk] for drink) until a decidedly later stage (Stampe 1969). The

point that Natural Phonology wants to make here is simply that the child has mentally represented these basic phonological oppositions considerably before he can actually pronounce them. The Language Acquisition Hypothesis, then, extends the Phonemic Hypothesis to encompass evidence derived from the child's earliest formative grammar as well.

3.1.1.5 The Phonetic Change Hypothesis

Stampe's theory of phonetic change draws from and expands upon Passy's nineteenth-century arguments that change arises directly from an "imperfect imitation, by children, of the speech of adults" (Stampe 1969, page 448). When some natural process that does not apply in the standard speech is no longer being suppressed by children acquiring their language, then a phonetic change is the observed result.

Stampe's view of phonetic change follows reasonably enough, then, from a conception of phonology as both learned rules and innate natural processes and from a connected assumption that much activity in the acquisition of speech patterns is the suppression, limitation, and reordering of these processes of the innate system. When a child fails to master a sound, it will quite logically appear that he has changed it for some sound he is regularly substituting in its stead; and when the child fails to suppress processes, the surface reflex will just as naturally be interpreted as the simple addition -- rather than the resurfacing -- of a rule. The desire for conformity and the ubiquitous pressure of habit limits effectively any large-scale innovations by children and rejects most that do occur. The innovations that are admitted by the adult system are highly conditional at first and phonetic change begins with optional but never

obligatory pronunciations (Stampe 1969).

It is just this view of phonetic change fostered by the natural phonologist which will ultimately account for (1) why such changes are not observed as well with the adult speakers of any known dialect (Kiparsky 1965, page 32), (2) why change may appear in some instances to be quite radical, and (3) why phonetic change is at the same time marked by such awesome regularity.[9] It is simply that children will deform adult speech only in a manner that is -- however radical -- always exceedingly regular.

Let us now take up in more detail some further subtleties of these various hypotheses, along with several closely related issues. We will begin with an elaboration on the interpretation just presented of a theory of phonetic change.

Our starting point is an example Kiparsky elects to illustrate what he takes as the expansive explanatory power of a "process" type approach (in a Kiparskyan sense and not a Stampean one) to morphophonemics associated with generative grammar over either the completely "abstract" (Lamb, Fudge, Stratificational Grammar, etc.) or the completely "concrete" (Item-and-Arrangement) approaches. This example proves to have somewhat ironic implications, however. For on closer inspection it seems to reveal even further explanatory advantages in Natural Phonology, over and above the generative phonology which is justified by Kiparsky and which is limited to the claims of the standard model.

The Slavic Palatalization example to be pursued is to be found in both Kiparsky 1968a and Kiparsky 1968b. In Russian the dentals t, d, and s and the velars k, g, and x are all palatalized to č, ž,

and $\underline{\check{s}}$ by ordered rules of Palatalization (t,d,s and k,g,x → $\check{c}$,$\check{ʒ}$,$\check{s}$ respectively) and Spirantization ($\check{ʒ}$ → $\check{z}$). A first step is the rule of Palatalization which converts the dental and velar stops and continuants into the correponding strident palatals. An affricate $\check{ʒ}$ which is a palatalized reflex of d and g becomes the spirant $\check{z}$ by the second rule of Spirantization. Yet in Ukrainian and also in Belorussian, Kiparsky notes, the system is changed and those $\check{z}$'s which are synchronically from underlying d return to the affricate $\check{ʒ}$. There is no further change in other $\check{z}$'s of this system. Those which are synchronically derived from /ɣ/ (= Slavic /g/) or resulting from the underived forms display absolutely no change. The explanation offered by Kiparsky (and certainly the correct one) is that $\check{z}$ from /d/ but not from /ɣ/ or underlying /$\check{z}$/ reappears as a phonetic stop precisely because it is the only form that was already a stop underlyingly. That is, the change here is a loss in Ukrainian and Belorussian of the second (Spirantization) rule.

The original underlying forms, rules, and subsequent phonetic change in this system may be summarized with Figure Seven. Loss of the rule of Spirantization permits the step in the derivation (Step Two) at which Palatalization has applied to emerge as a new phonetic representation.

(2) FIGURE SEVEN

UKRAINIAN AND BELORUSSIAN PALATALIZATION

STEP ONE	Underlying Forms	biɣ	sid	mež
STEP TWO (new output)	Palatalization	biž	siž	mež
STEP THREE	//Spirantization//	biž	siž	mež
STEP FOUR (original phonetic output)	Additional Rules	bižu	sižu	meža

UKRAINIAN FORMS: bižu "I run"

sižu "I sit"

meža "boundary"

Kiparsky's purpose in citing this case of "rule" loss is avowedly to establish the explanatory power of a generative phonology, which regards morphophonemic representations as abstract entities having intrinsic phonetic realization and which also regards linguistic change as being a change within the abstract "rules" of the grammar. The change is a change in the supporting morphophonemic system itself. An Item-and-Arrangement theory of grammar identified with the concrete approach to morphophonemic alternation does not claim a single and systematic change within the system at all, but rather a simultaneous change of a large number of isolated morphemes. Under this system, regularity of change is lost and no connection is ever made between alternations in one morpheme and those which occur in another (see Kiparsky 1968a, page 7 and 1968b, page 12).

Kiparsky has also demonstrated that an abstract approach to morphophonemics which accepts morphophonemes with no remaining spectre of phonetic reality is even less explanatory and more arbitrary still. It is suggested that such a system might well posit rules like the following (Kiparsky's Rules (1), (2), and (3) respectively):

(3) a) % → /ž/ in palatalizing environments
b) /d/

(4) a) * → /ž/ in palatalizing environments
b) /ɣ/

(5) # → /ž/

with %, *, and # taken as the arbitrary abstract morphophonemes. Here the change is to be written as follows: "change the right hand side of subrule (2a) from /ž/ to /ǯ/" (Kiparsky 1968a, page 8 and 1968b, page 13). The motivation for an affrication of the /ž/ derived from

/d/ is, however, lost once its underlying representation as a phonological stop is ignored. Also given up is the appearance of a [ž] "at an intermediate stage in the derivation," a further motivation for the actual subsequent change. In short, once it has been denied that these morphophonemes or any other phonological representations have a phonological characterization and will appear as intermediate representations, all hopes for any real explanation in our system of phonology are irretrievably gone.

Kiparsky's defense of "process" phonology is in the main viable and admirable. Still, a rather prominent difficult remains and this is that for all its advances generative grammar is not nearly as explanatory as we might hope. What, after all, is the motivation for a rule loss of the kind we have just illustrated? What do we learn from such examples about the speaker's performance capabilities or about the structure of a grammar? It seems generative grammar can offer us no convincing account of why most such loss occur (though a few papers like Miller 1973 do offer substantial inroads into the motivations for certain types of universal "phonological" change). It is uncontroversial by now that we may not -- as once was naively hoped -- appeal to any obvious evaluation metric based on assumptions about simplicity criteria. Kiparsky's work (offering conditions of rule opacity and rule transparency) is sufficient in itself to affirm that the appropriate evaluation measure will not be a mere feature-counting or rule-counting device. And it is also to be kept in view that Kiparsky's more or less "concrete" approach to morphophonemics requires an even greater use of diacritics (e.g. the rule-feature proposal in Kiparsky 1968a) and is in some senses more

complex -- however more or less explanatory -- than an uninhibited "abstract" analysis.

Also, the assumption of generative phonology that one mechanism of change is the addition of rules to the grammar (the others being loss, generalization, and complication of rules) seems largely at odds with any true criterion of simplicity. If the child constructs rules to account for his linguistic experience and if an evaluation measure is the simplicity in the numbers and structures of these rules (that is, simpler and thus better or more highly valued grammars will have fewer and less complex rules), then it is altogether unapparent why any new rules should ever be added to the grammar in the first place, or why any existing rule should ever be further complicated within a grammar which ideally changes only in the direction of a heightened simplicity.

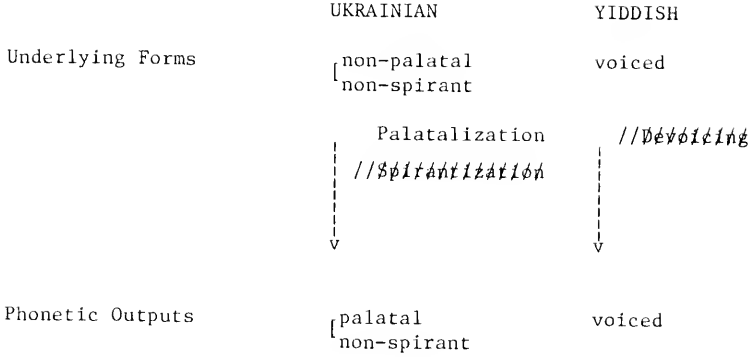
Among generative phonologists, Miller (1973) has come to grips most directly with this problem. Miller observes that current theories see linguistic change as of potentially two types: an addition of rules and subsequent simplification, or an elaboration of rules and then simplification. But for Miller (as I suspect for anyone else who has contemplated it) this is hopelessly uninsightful. What else might logically be expected to happen -- other than the addition, loss, and reordering of the rules in the grammar? The question still remaining is: why? What is the function of these "rules" and how do grammars pick up "crazy" rules, reorder them, and even eventually discard them again just as randomly?

If one assumes as an alternative the Stampean position which has been outlined in this and preceding chapters, then many of these questions simply evaporate. One alternative explanation of linguistic

change is that "rules" are not added or struck from the grammar at all. Rather, forms subject to natural processes are allowed to again surface (thus giving appearances of new rules or processes) once the processes earlier removing them are now no longer wholly or partially restricted.[10] We might best capture this notion of phonetic change with the schema suggested in Figure Two -- again referring here to the examples from Ukrainian (page 163 above) and Yiddish (page 159 above) which have already been discussed in a more traditional light.

(6) FIGURE EIGHT

RULE LOSS IN UKRAINIAN AND YIDDISH DIALECTS



Ukrainian thus yields to this type of Stampean explanation, with Spirantization meeting all the specified characteristics of a natural phonological process (as I have summarized these above in Section 2.2). in Ukrainian and in Belorussian a generation of new speakers apparently have elected to restrict a natural process of Spirantization. Again considering the nature of processes (phonetic, optional, and minimal substitutions) as well as rules (learned, abstract, obligatory, and radical substitutions), and the mechanisms for limiting processes elaborated on in Section 3.1.1.2, such an interpretation suggests insightfully just how the suppression or limitation of the regular substitutions ("processes") in a language might well occur. Notably Kiparsky's modification of the standard view of linguistic change encompasses a nearly parallel explanation (however, see note nine and note ten below):

> We have very little empirical knowledge of how, in practice, such loss takes place. Two of the special properties of this kind of change, its simplificatory character and its slow diffusion, strongly suggest that its origin must be sought not in mature speakers but rather in the process of language acquisition. In particular, the following way in which loss might arise suggests itself.....
>
> Suppose that the child has constructed the optimal, "simplest" grammar for a certain body of linguistic experience. Further data that he encounters may subsequently motivate a reanalysis. If no reanalysis is made, the grammar will "congeal" as the child matures and passes the stage where complex unconscious skills are easy to acquire. That no reanalysis is made may be due to the fact that the language in fact has no counter-data. In this case the child has arrived at the "right" grammar. But -- and this is of vital importance -- that is not the only possible reason why a reanalysis may fail to take place. It may well be that the language has counter-data which do not register on the child in the language-acquisition stage, perhaps because of their rarity, the child's limited linguistic experience, some mental or perceptual limitations of the child, or probably most commonly just because of the inevitably fragmentary and incomplete nature of the data at the child's disposal. In that case, linguistic change has taken place: the child arrives at the "wrong" grammar, i.e. not the grammar of those whose speech

provided his linguistic experience (1965, page 32).

If "rule loss" is in actuality either the loss of a process (where processes have previously been misinterpreted as being learned rules) or addition of a process (which works to undo the effects of earlier substitutions), and if what has been misnamed "rule addition" comes about through what is actually the suppression of processes rather than the multiplication of rules, then it is equally reasonable that changes through "rule generalization" are also mere appearances which follow from the similar restriction of limitations on the processes once fully active in the grammar. A particularly illustrative example of how what is interpreted as rule generalization by some linguists might better be explained as loss of a process is seen when we take up one recent paper by Daniel Dinnsen (1974), to which I will turn next.

Dinnsen would contend that the axiomatic principles of linguistic change -- labelled rule addition, rule loss, rule generalization, and rule complication -- are subject to reinterpretation within the confines of his own version of generative theory. He wishes to show that all reported cases of rule generalization and rule complication are derivable by deductive procedures from the simpler and independently motivated principles of rule addition and rule loss.

Dinnsen appears intent on constraining sound change through reducing its motivations by half and assumes that this will, in turn, explain such apparent "facts" as why rules are generalized at the end of a grammar (since the phenomena of generalization are taken to be those of addition and rules purportedly are added only at the end of grammars, based on the observations of King 1973, 1974) or why only opaque rules (Kiparsky 1973a, Kaye 1974b) become more complicated and why rules

complicate (assuming that they do) in precisely the way that they do. I will take up here only his first example, that of generalization, in order to establish the areas of misinterpretation. Again, we have an example here based on the devoicing of obstruents in German.

Dinnsen repeats the standard proposal that one mechanism of rule generalization is the loss of a feature specification which removes a restriction from the rule and allows its wider application. It is by this means that we can illustrate a development of Final Devoicing in German. We might first assume (as has traditionally been done) that initially German had a rule given below as (7) which accounts for the voicelessness of fricatives in word-final position. When this rule is later generalized to apply to all obstruents in the system we could assume the rule is then to be formulated as (8) below. The increase in generality is therefore claimed to be a function of unconditioned and spontaneous loss of a single relevant feature.

GERMAN DEVOICING RULES

(7) FRICATIVE DEVOICING

$$\begin{bmatrix} +\text{obstruent} \\ +\text{continuant} \end{bmatrix} \rightarrow [\ -\text{voice}\ ] \ /\ \_\_\_\ \#$$

(devoices fricatives word-finally)

(8) TERMINAL DEVOICING

$$[\ +\text{obstruent}\ ] \rightarrow [\ -\text{voice}\ ] \ /\ \_\_\_\ \#$$

(devoices all obstruents word-finally)

(9) STOP DEVOICING

$$\begin{bmatrix} +\text{obstruent} \\ -\text{continuant} \end{bmatrix} \rightarrow [\ -\text{voice}\ ] \ /\ \_\_\_\ \#$$

(devoices just stops word-finally)

Dinnsen would have us believe that cases like this of defensible rule generalization are handled better if understood as the addition to the grammar of a conflatable rule which is a "complement rule" to our original less general rule. Under this hypothesis we are to assume that Rule (7) was not converted to Rule (8) by simple loss of a single feature, which is merely descriptive of what happened but not at all explanatory. Rather, Rule (9) above supposedly has been added to the grammar of German at this period and then, by the standard notational devices, collapsed with Rule (7) to yield (8). This presumably results in an explanation for how a feature which marked fricatives alone as subject to the rule was somehow regularly lost.

Dinnsen warns that this may well look like a notational trick; yet is is claimed to be an explanation that is both reasonable and viable, since at a period in the history of German corresponding to this change (circa 8th Century A.D.) the fricative rule (7) was still obligatory and the obstruent rule (8) extant but optional (cf. Dinnsen 1974, page 4). Dinnsen takes this to mean that two rules existed but that they were not yet conflatable due to their relative status as obligatory and optional rule.

This last supposition hardly appears to bear validity. The fact that devoicing was obligatory for some forms and optional for others is no more evidence for independent rules than it is for the shifting status of a single rule or (under the proposal Dinnsen rejects) the optional rather than complete loss of a single feature "continuant".

The real difficulty lies in Dinnsen's implicit argument that such a rule addition theory is plausible at all, without serious violation of what he (like generative phonologists at large) takes to be an

axiom that all linguistic change is change prompted by simplification. This example meets the same difficulty as Kiparsky's Ukranian example, or any of hundreds more where rules appear to arbitrarily come and go. How is it that a second rule is added in the first place, if the grammar is ideally sensitive in the direction of increasing simplicity?[11] I seriously question whether Dinnsen would want to contend that the addition in German took place just so that conflation with some already existing rule might potentially occur.

More troublesome still, and at the very root of the issue, is Dinnsen's failure to accept Devoicing for what it is: i.e. a physiologically based natural process a behavior in language systems distinct from that of abstract and arbitrary rules. Once we accept this natural status of Devoicing as a universal constraint aimed at reducing possible articulatory difficulty, it is no longer at all mysterious why there should be pressures within the grammar for the spreading of its domain -- from final fricatives to final stops and eventually, perhaps, to all obstruents in all positions. The occurrence is among the most natural types that could be expected in phonetic change.

It is not implied here that there are not also instances of change occurring among the phonological "rules" or even among the morphological "rules" of a grammar. This is undeniable and well documented (see the excellent discussion in Miller 1973 e.g. of the respective roles of "natural" and "less natural" rules in determining "nonfunctional" stylistic variations, and of the role as well of morpho-syntactic considerations in the manifestations of sound change). But much "phonetic" change, whether or not it is accompanied by corresponding "phonological" changes, seems more insightfully accounted for within a theory sees

such changes as resulting not from simplifications in the adult grammar, subsequently thrust upon the speech patterns of children, but from relaxations of constraints imposed by the child on the language he is learning and then retained in his more mature speech outputs. Such an account of at least one form of linguistic change would also feature an added attraction of explaining a currently popular proposal within generative grammar that rules are added only at the very end of the grammar -- an assumption that would necessarily follow naturally from the several related facts that (1) "rule addition" is in reality a manifestation of the addition or suppression of natural processes (which can be added in the sense that previously suspended processes are now no longer so) and (2) processes in turn follow rules and are located at the extreme end of the grammar. Robert King's thesis that rules are added through linguistic change "at the end of the phonological rules but prior to the low-level phonetic rules" (1973, page 551) is indicative only that the latter category is not at all equivalent to Stampe's notion of natural processes, which would always include a good many of the former (e.g. Palatalization, Flapping, Spirantization, Vowel Epenthesis and Vowel Deletion, Nasal Assimilation, among others) as well as the latter kind.

3.1.2 The Uniqueness of Natural Phonology

Kiparsky's theory of process morphophonemics reveals very little about the functions of individual rules in a grammar. The respective and contrary functions of rules and processes become the overriding concern of Natural Phonology, however, as a contrasting explanatory approach to phonological theory.

Discussion in Natural Phonology to date, on the other hand, has been rather narrowly restricted to a single type among the phonological processes. These are the syntagmatic or context-sensitive processes, which taken together make up a majority class of substitutions imposed by speakers on their individual languages. The smaller set of arbitrary and learned rules has been only rather fleetingly described by the natural phonologists (see Stampe 1973a, Chapter Two; also Wojcik 1976, which explores important distinctions between rules, processes, and a category of "suppletions"), and then for the sole purpose of a contrast with the genuine innate processes brought up for discussion.

Whereas Stampe emphasizes the role of these late "phonetic" type processes, phonologies patterned after SPE to a comparable degree seem to ignore phonetic-based rules and elect instead to formalize highly abstract rule-based grammars. For the SPE prototypes, phonetic detail rules are more or less taken for granted, as in Stampe's work the "P Rules" seem to be, and thus never properly distinguished in form and/or function from rules governing abstract alternations. A spirantization rule, cluster simplification rule, or palatalization rule, all with evident physical motivation, are taken as part and parcel of the same block of rules converting phonological matrices to phonetic matrices as, say, a less universally evidenced Velar Softening rule or an abstract and highly cognitive stress rule. Generative phonology obviously faces this paradox on two fronts: there is no more empirically motivated distinction between what are "phonological" rules and what are called in SPE "lexical redundancy" rules or "morpheme structure conditions" (e.g. the rule devoicing stops after syllable-initial /s/ in English is a constraint on

lexical representation as well as an automatic fast speech process oblivious to grammatical boundaries) than there is between phonological and phonetic detail rules. (This unwarranted complexity of generative phonology receives special treatment in Chapter Four, viz. in Section 4.1.1.) In this respect at least, then, Stampe's partial model appears to merge with (or at least complement) the standard phonology. A hasty assessment of Stampe's work might even be that it does no more than to supply some needed constraints on uncontrolled abstractness in the current versions of phonology. Like Halle's recent efforts at formulating a morphological component for generative grammar, Stampe's definition of phonological processes whittles away at some of what has been hastily lumped together as SPE's amorphous and unmanageable "phonological" component. This view might lead one to conclude that Stampe's work on Natural Phonology differs more in emphasis than in substance from the more traditional approaches to generative grammar.

Yet any attempt to view Natural Phonology in this way, as a close variant of the classical generative theory, is bound to misapprehend its essential motive. Natural Phonology is unique in at least three separate ways that follow from the discussion in the first sections of this chapter.

First, the Phonemic Hypothesis on which Natural Phonology rests demands the existence of three levels of phonological representation, proposing the addition of a level that has been judged not only redundant but logically impossible by earlier studies. Second, a dominant role in the phonology is assumed to be the special property of innate restrictions on speech and not of learned rules. The diametrically opposed assumption, that phonetic processes are complex-

ities the child must learn, is not only a feature of synchronic generative linguistics but also the sustaining principle of generative theories of grammatical simplification and linguistic change. And it is this abandonment of traditional assumptions in historical phonology (that children add rules instead of suppressing and limiting universal processes) which marks the third feature in which Natural Phonology radically departs from phonology as it is interpreted within the generative school.

That these processes are not merely the phonetic detail rules of the generative model is illustrated by their appearance under three distinctive guises.

Dominant paradigmatic processes or context-free processes of the type discussed briefly at the end of Section 2.3 above apply unrestricted in all languages to filter out ("block") impermissible segments from the speaker's lexicon of "native" forms, and they are rarely if ever extended to affect surface forms in adult patterns of speech. Since the function of paradigmatic processes, as Stampe defines them, is to intensify the distinguishing features of individual segments (Donegan Miller 1973b), while syntagmatic (context-sensitive) processes ease the difficulties encountered in sequences of segments (i.e. effect surface utterances and especially apply to rapid or unguarded speech), it follows directly that the former should be ordered prior to the latter in the normal case. A dominant process eliminating nasal vowels underlyingly in English (Chapter Two, Rule (20), above) does not restrict them phonetically (the evidence that this particular paradigmatic process does exist for speakers of English is effectively presented in Stampe 1973a, page 18 and following). Of course, the

fact that the effects of such processes in the adult mode of speech are almost always obliviated by later contrary syntagmatic processes makes them somewhat difficult of verification. Yet in the earliest phases of child language, or in the adult treatment of intolerable foreign segments during active cases of borrowing, their role becomes more readily observable and less subject to untestable hypothesis.

Morphophonemic syntagmatic processes which neutralize underlying distinctions by supplying alternative forms also permissible in underlying representation are also evident. These stand in sharpe contrast to later allophonic syntagmatic processes , like contextual vowel nasalization, which re-establish sounds already eliminated from the lexicon by prior paradigmatic processes (Stampe 1973a, page 27).

Cases we have already superficially examined provide the needed evidence for such a tripartite division of labor among the processes. We might compare the example of Yiddish final obstruent Devoicing (page 159 above), this time to the English contextual process of Vowel Nasalization. We recall that once the devoicing process has been lost by speakers of Yiddish, forms like [vɛk^{h}] (/vɛk/, /vɛgɛs/) resurface as [vɛg^{h}] "road". Yet [avɛk^{h}] "away" continues to retain its final voiceless stop in the face of such change, suggesting quite straightforwardly that /avɛk/ has underlying k (i.e. a segment which is specified among other things as [+obstruent] and [-voice] in the phonological matrices). The question inevitably arises as to why this second form is not now also analyzed by speakers as systematic phonemic /avɛg/? And the answer certainly can not be that the surface form is invariant, for this would also be true of the English form [kaet] for can't which is never taken to verify the underlying

presence of an English nasalized vowel (cf. Stampe 1973a, page 18 for discussion). The only really convincing answer would seem to be that in English there is a dominant process which rules out underlying nasal vowels, while no such process can be available to block voiceless obstruents at the abstract level (in the lexicon) for Yiddish (or for German). The final k of Yiddish can therefore be taken as it appears, at face value, but nasal vowels in English certainly may not. A process which would appear to introduce voiceless obstruents for segments that are lexically voiced in the earlier history of a language (what would happen, of course, is that a new generation of speakers would simply stop making the extra articulatory effort needed to voice these segments in this position) would be morphophonemic in the traditional sense of processes having the property of neutralizing underlying contrasts, but vowel nasalization in English retains strict allophonic effects.

We are now left with the following set of definitions. Paradigmatic or Dominant processes are potential and sometimes active processes in the phonological system of a language which account for the non-occurrence of particular segments in underlying form. Morphophonemic processes are contextual substitutions which neutralize underlying contrasts. Allophonic processes are contextual substitutions as well, but those which introduce at the phonetic level segments ruled out by paradigmatic processes at the underlying or phonemic level and which therefore are never capable of neutralizing any underlying or deep level contrasts.

As these definitions should establish, three principles would appear to control choices of underlying representations in the analysis

adopted by Natural Phonology (this point has been elaborated in both Stampe 1973a and Hooper 1975). (1) Underlying segments must be only as abstract as is necessary to account for pronunciations of all "naturally derivable" surface forms, where the term "naturally derivable" is taken to mean derivable through the applications of natural processes. (2) Underlying forms are fully specified for all features and no archi-segments are permitted (this following of course from Stampe's most basic presumption that all representations at all levels in the phonology are pronounceable). (3) Segments must be barred from underlying representation if either (a) they are derived exclusively from the allophonic processes, or (b) there is evidence they should be eliminated from the lexicon by application of some dominant context-free process. Processes of the final kind, as we have been suggesting, are partly to be taken on faith, though they are undeniably evidenced in children's earliest substitutions and occasionally glimpsed in the various strategies of loanword phonology.

To make this uniqueness of Natural Phonology more obvious still, I would like to turn now to a closer examination of what I have called the Phonemic Hypothesis of Natural Phonology and to some of its apparent claims and implications.

The Abstractness Controversy in current linguistics has been largely preoccupied with abstractness in essentially two guises. The first is that captured by Kiparsky's Alternation Condition, which specified loosely that underlying segments in a morpheme must be realized phonetically in at least one surface alternant of the form and which rules against purely diacritic use of phonological features or phonological use of diacritic features (Kiparsky 1968a). A second type involves

the depth of representations and leads Stampe (1968) to conclude it is a traditional phonemic level (which makes the crucial distinction between morphophonemic processes and allophonic processes as those which may and may not constrain phonological representation) that is most relevant to linguistic description. In formulating the Phonemic Hypothesis Stampe answers both concerns with unwarranted abstractness. However, since he does so at the cost of adding a third significant level of representation to the phonetic and systematic phonemic levels already utilized in generative descriptions, most controversy over Stampe's model will understandably involve a justification of this added level.

It is necessary to approach more directly at this point exactly what Stampe seems to have in view regarding the phonemic level of representation. His notion seems to fall closest to that of classical phonemic theory and in Stampe's own terms "the view of underlying representation that emerges is one somewhat resembling that of Sapir or Bloomfield: underlying representations are basically phonemic, with 'morphophonemes' like the German final voiced obstruents included in individual morphemes whose alternants require them" (1973a, pages 29-30).

The phonemic theory of Sapir or Bloomfield is a variant of the American Structuralist theory which assumes a "biuniqueness" condition (see Section 2.3 for discussion) requiring that the phonetic representation be always uniquely determined by phonological representation and in turn that the phonological representation also uniquely determine phonetic forms. The deepest level meeting this condition was assumed to be the classical phonemic. Deeper representations were

determined to be morphophonemic as they demanded access to morphological information for their recovery. This view of phonology takes it as axiomatic, then, that there is a unique system of morphophonemics which maps underlying (morphophonemic) onto phonemic representations, as well as a unique system of allophonics which maps corresponding phonemic onto phonetic representations.

The question that inevitably arises, as we have simply glossed over it above, is whether this is not after all, as Chomsky and Halle have independently suggested, merely a redundant distinction as well as an unjustified duplication of levels. It turns out, however, that just such representations do appear to hold tangible reality for speakers. And the evidence for this is of at least two independent and persuasive kinds.

The type evidence supporting a phonemic level which Stampe offers in Stampe 1968 (where he first proselytizes his notion of a phonemic representation) is of a strictly historical nature. This is the issue of Yiddish Final Devoicing already elaborated on at several points. Stampe argues further (in Stampe 1968) that Halle's famous dictum against any version of the biuniqueness condition, on the basis that a distinction between morphophonemics and allophonics would obscure the nature of processes which seem to have both functions simultaneously, seems hardly convincing since generative phonology (of the type to which Halle subscribes) proposes a similar and equally invalid division between phonological rules (feature-changing rules) and lexical redundancy rules (rules which fill blank matrices). Spanish Nasal Assimilation brought up in Section 4.1.1 is sufficient to underscore this formidable obstacle facing the generative model. The

point to be taken under consideration here, however, is that despite these difficulties facing both theories in identifying discrete blocks of rules, there is considerable evidence at hand that speakers have real levels of representation corresponding to both SPE's systematic phonemic level (taken loosely to mean "lexical representation") and Stampe's (along with the classical phonemicist's) phonemic or shallow level.

Stampe defines a "phonemic representation" as that which exists for speakers in cases where processes of neutralization apply (see the passage quoted above on page 159). Whether we take neutralizations to be of a type "A becomes C" or of a type "A and B both become C", in either case these neutralizations must be of a type where C already is an underlying segment of the language. Viewed otherwise, they would contradict Stampe's clear notion of allophonic and morphophonemic processes (see note sixteen and note nineteen in Chapter Two). Here the issue is: which types of phonological substitutions will actually constrain a phonological representation (in the sense of determining what must be the legitimate underlying segments and what must not)? SPE theory contends that no processes making actual substitutions (only redundancy rules) provide such a constraint; within Natural Phonology it is the division between the morphophonemic and the allophonic processes which is exclusively relevant. The result of such an assumption for SPE is that where alternations do not expose all features of a phonological representation these unspecified features must be interpreted as "archi-segments" in the phonological matrices (SPE, page 166). A resulting assumption for Natural Phonology (given that all phonologically neutralized features are represented

as though the neutralizing processes had already applied to them) is that such segments are fully specified and specified precisely with their "unmarked" values. To say this differently, they are represented phonemically.

The existence of two independent types of evidence for phonemic representations has been mentioned above and it has been established that the first type of evidence is all Stampe presents to date, viz. that in cases like historical Yiddish (when neutralization processes are lost via the suppression of a process) it is the "unmarked" segment which universally reappears. The most noteworthy feature of the much-cited Yiddish example is simply that when obstruent devoicing has been suppressed, in the forms where there was no previous alternation ([ap^{h}] and [avɛk^{h}]) there is also no similar resurfacing of the voiced obstruent. An explanation in view of the Phonemic Hypothesis is that in these cases there was no morphophonemic process to be lost. Since there was no alternation, what were once voiced underlying obstruents long since had been relexicalized by process of analogy.

A second type of evidence suggesting the substantive reality of the phonemic level is of an equally impressive order. This would be the conscious awareness speakers demonstrate of what they take as being allophonic processes in their phonological system; or, to put it only slightly differently, the awareness they maintain of phonological representations as they stand after the application of all but the purely allophonic substitutions. For purposes of clarification, this might be labelled the level of "idealized" representation.

In an example which is highly relevant here, Shibatani (1973, page 95) reports that when speakers of German are presented with

"nonsense" forms of the type [bund], [rād], or [līb] (Shibatani's broad phonetic transcriptions) -- forms to which the standard German process of Word-Final Devoicing has not applied -- these forms are then rejected as improper German forms, despite the absence of any such constraint at the lexical level. This would seem to be not so much a case of a surface level constraint (Shibatani maintains an SPC) since there appears to be no evidence that speakers would make the same kind of rejection in the cases of absent (not applied) allophonic processes as opposed to the morphophonemic processes involved in the case he cites. What seems to be at stake is the speaker's evaluation of what is "phonemic" in his language and not his judgement about what is properly phonetic.

My own research with Cuban informants has so far turned up some parallel and equally provoking examples. In one case involving a process we will elaborate on in Chapter Five, Cuban speakers uniformly apply Obstruent Velarization to achieve forms like aberrant "se[k]tiembre" and "conse[k]ción" for the standard Spanish renderings "se[p]tiembre" and "conce[p]ión". This is a morphophonemic process as the voiceless velar stop in this position is a perfectly acceptable underlying segment of all Spanish dialects. What takes on an added significance in this example is that in casual speech these same forms are almost always heard produced with glottal stops, as in "se[ʔ]tiembre" or "conse[ʔ]ción" (an observation which I owe to the astuteness of Bohdan Saciuk). Further, when asked what he is uttering, the informant will tell his inquisitor [k] not [p] ("that would be the way they say it in Spain" is a frequent comment); he never offers that it is [ʔ] (about which the linguistically naive

speaker knows nothing of course). Again, the speakers seems to be demonstrating here a very real and even conscious awareness of an ideal or phonemic level of representation.

But the most irrepressible cases are those which arise from the study of loanwords in Chapter Seven. Here it is verified that in analyzing foreign borrowings speakers will adopt the foreign phonetic forms where possible as native phonemic forms -- the surface form minus all and only the allophonic processes which they undergo. There is no firmer evidence that speakers are aware in some real sense of what are the allophonic processes of their grammars and what is a phonemic representation to which these processes all apply.

3.1.3 Two cases of reanalysis

In this section I have been illustrating and defending certain principles of Natural Phonology which I have chosen to refer to as the Phonemic Hypothesis, the Innateness Hypothesis, and the Phonetic Change Hypothesis, among others. As a practical illustration of the role of processes in reinterpreting generative phonology, I turn now to an unresolved issue from James Harris's seminal treatment of Spanish generative phonology -- the writing of the glide formation rules.

I will also take up here a recent proposed solution to Spanish glides by William Cressey (1974). It will be shown at this point that the view of Natural Phonology offered throughout this chapter suggests at least one promising solution to the paradox of glide formation which Harris has left unresolved, as well as a marked improvement on Cressey's attempts at a reanalysis.

In Harris's Spanish Phonology (1969) a general glide formation rule is given as:

(10) SPANISH GLIDE FORMATION RULE

$$\begin{bmatrix} V \\ +\text{high} \\ -\text{stress} \end{bmatrix} \rightarrow G \; / \; \begin{Bmatrix} \_\_\_ \; V \\ V \; \_\_\_ \end{Bmatrix}$$

Harris is then forced to conclude that some Spanish glides must be introduced at the systematic phonemic level of underlying representation and are therefore not products of this standard glide formation rule. His evidence is of two fairly impressive kinds.

First, there are verbs of the type of cambiar, containing a stem-final glide (cf. [kambyo], 1st person singular, "I change"), which will contrast with those of the type of ampliar ([amplio], "I expand"), which have stem-final stressed vowels. Both classes of these verbs are considerable so we are not dealing here with isolated exceptions. Secondly, there are words which appear to show stress on the fourth from last orthographic vowel -- a condition otherwise impossible in Spanish -- unless the segments written as u and y are not vowels at the time stress applies (Harris 1969, page 31). A partial list of these would be: laudano, naufrago, aulico, caustico, alicuota, conyuge, pleyade (with stress on the underscored vowel).

Harris sees one initially plausible alternative to including glides in the inventory of systematic phonemes of Spanish. Of course, there is no absolute mandate for avoiding this (Harris admits to being aware that SPE considers VG and GV sequences actually less marked than VV sequences); it would, however, mean a significant departure from traditional treatments of Spanish phonology. Underlying glides would be a unique claim of which Harris is modestly cautious. The

alternative, then, is to propose a second, structurally similar glide formation rule, with the difference of the single deleted feature [-stress] which would now allow the rule to come prior to stress assignment. But again there are two weighty arguments to block such a proposal. Two nearly identical rules in the grammar, which are not ordered contiguously and therefore not capable of collapsing, is not at all enticing, given the standard theory of ordering. Perhaps more fatal still to such an analysis, Harris points out that words like país and baul would somehow escape the first of the two glide formation rules. That is, pairs like pais-paisano (again stress on the underscored vowel) and baul-baulero demonstrate that apparently some glide formation, at any rate, must follow the determination of stress placement. The two-rule proposal is therefore to be dispatched (Harris 1969, page 32).

Cressey tackles this issue by electing to return to this first solution Harris rejects and adopting two rules of glide formation for Spanish. Since it is strikingly obvious that the two rules involved are quite different in nature, Cressey conjectures that each must maintain a very different status within the grammar. One rule (that replacing the pre-stress rule Harris finds intolerable) is suggested as a Marking Convention, which would work as a morpheme structure condition and apply to "lexical" matrices filling in the feature specifications needed in the resulting "phonemic" matrices. This two-part convention would establish that for Spanish the unmarked value of a feature [syllabic] would be minus whenever the segment involved is a high vowel adjacent to another vowel, and otherwise that the unmarked value of this feature is opposite to the segment's

value for the feature [consonantal]. This convention would operate in conjunction with one specifying the unmarked value of the feature [consonantal] as plus word-initially and opposite to the preceding segment in all other cases.

In this manner the phonetic glides in words like áureo and náufrago would be provided in the systematic phonemic matrices (though still unspecified in the lexicon) and the high vowels in país and baul would escape glide formation as they could be lexically marked as [m SYL] and conveniently passed over by the Marking Convention which Cressey adopts. To offer such a solution Cressey must make at least the following assumptions: (1) that marking conventions per se apply after the spelling out of morphemes as words (a potentially controversial point though one we need not elaborate on here as it is irrelevant to the issue at hand); (2) that native speakers of Spanish do not learn words like náufrago as containing glides (that is, that glides appear at the systematic phonemic level before application of any phonological rules, but not in the lexicon); and (3) that words like país are marked in the lexicon as exceptions to the universal convention. The cost of two identical (or nearly identical) glide rules is thus replaced by (traded off against) the cost of special lexical markings.

Cressey's second rule for glide formation is established as a "variable" rule accounting for optional glide formation in the cases of two adjacent non-high vowels or high-vowel and vowel sequences formed in rapid speech, often with resulting mergers of individual words. Here variant pronunciations are possible (as is never the case with normal glide formation, giving always [bawlero] and never

[baulero]) and Cressey proceeds to give several examples:

UNDERLYING SYLLABLES	RAPID SPEECH	ORTHOGRAPHY	GLOSS
te - a - tro	tya - tro	teatro	theater
po - e - ta	pwe - ta	poeta	poet
to - a - lla	twa - lla	toalla	towel
mi - a - mor	mya - mor	mi amor	my love
tu - e - dad	twe - dad	tu edad	your age
te - a - do - ro	tya - do - ro	Teodoro	Theodore

That there is a rapid-speech glide formation process that involves non-high vowels and will apply across word boundaries and is therefore a very special case of normal glide formation is beyond doubt, and Cressey's formal recognition of this variable rule improves upon Harris's limited single-rule approach. But there remain other facets of Cressey's article which we find less palatable.

Cressey speculates, for example, that variable rules like the late glide formation rule and functionally similar rules which are "absolute" and not at all optional, such as the glide formation Marking Convention, must be somehow manifestly related. But this leads directly to the paradoxical concluding observation that the absolute glide convention "must surely have started out as a variable rule" (1974, page 10). In support of this we are shown an early stage in historical Romance languages which saw shifts from three syllable patterns like ra-di-o to two-syllable-plus-yod combinations like that of ra-dyo. But I know of no precedent for this rather remarkable claim that phonetically-motivated variable rules might by virtue of an historical process become established as universal marking conventions. Such conventions are, after all, not a part of the grammar of any single

language but instead universal conventions which "interpret" grammars of individual languages. If Cressey is at all on the right track about the existence of two glide formation rules -- and I think it reasonable to assume he is -- then the decision to formulate one as a universal marking convention leads us directly to an insurmountable and unwarranted difficulty.

Of course the only real motivation for proposing a marking convention within Cressey's scheme is, once again, this problem of two approximately identical phonological rules, identifiable only by their ordering at separate points in the grammar. The need for such choice of conventions, on the other hand, causes Cressey at least three additional obstacles. First, this suggestion that it is possible to combine the universal marking conventions with language-specific morpheme structure conditions as a single set of rules relating lexical and phonemic matrices nowhere has any tangible support. Also, to assume, as does Cressey, that a fine distinction should be made between what is specified in a lexical matrix and what is specified in a related phonemic matrix may, by his own admission, have every appearance of mere "hair-splitting".[13] And as noted above, the result of any effort to relate a language-particular variable rule to a universal marking convention appears certain to lead to an implausible claim about linguistic change.

Natural Phonology would seem to rescue much of Cressey's proposal with only minor modifications. For what Cressey would call a "variable" rule is quite apparently a Stampean natural process. By Cressey's own formulation the earlier glide rule (i.e. marking convention), though suspendable by conscious effort (speakers could always say ba-u-ler-o

if asked to do so) since it has little physiological motivation and offers no restriction on pronounceability, is the normal case applicable irrespective of style or rate. The variable rule reflects the situation in which through increased speed and relaxed style glide formation even across word boundaries has increased physiological motivation. The possibility of a natural process being frozen in the grammar at a later stage as a learned "absolute" rule has sound precedent in the discussions of Natural Phonology (see e.g. Stampe's example of this type of development for the often discussed English Vowel Shift Rule in Stampe 1972).

This assessment of the two Spanish glide formation rules as an acquired rule and a context-sensitive process, rather than as a variable rule and marking convention, would still leave us with the question of the lexical status of glides unresolved. However the only motive for such a proposal originally was Harris's unwillingness, given a standard theory of rule ordering, to admit two uncollapsable though structurally similar rules. As I have been trying to show with this section, the avoidance of two rules by designating one as a marking convention which is a candidate for universal status is unnecessarily problematical. The recognition of the second rule as a natural process eliminates this duplication entirely.

A second nearly parallel case may shed even further light on these alternative types of analysis. Such a case arises in Daniels's study of the Russian consonant clusters (Daniels 1973). Demonstrated are nearly identical assumptions about the involvement of universal interpretive conventions (SPE marking conventions) among the optional fast speech rules. Daniels treatment is of the following Russian

consonant rules: voice-assimilation, back-assimilation, and palatal-assimilation. The issue once again here is the disappearance of certain persistent ordering difficulties once substitutions classified originally as Chomskyan phonological rules are reassessed as the natural innate processes.

Daniels observes that the three mentioned assimilation rules are all fed by outputs of rules which account for V/∅ alternations and thus presumably must follow such vowel-deletion rules. It also needs to be observed, however, that all three assimilation types occur (1) in non-derived lexical clusters (within morphemes), (2) in non-lexical clusters which arise through processes of word formation, and (3) in the non-lexical clusters arising through casual speech processes, as well as (4) in the non-lexical consonant clusters arising from the aforementioned V/∅ alternations.

A further observation Daniels is forced to make is that, given the Russian assimilation rules will operate on the output of the casual speech processes, it would seem necessary to assume they are of the sort referred to in generative phonology as "low level phonetic rules". This assessment remains unavailable though, in light of the operation of the same processes as lexical constraints (morpheme structure rules?) and obligatory substitutions in non-casual speech forms as well. As with Spanish glide formation, the only alternative solution seems to be two distinct sets of identical rules, an alternative not highly attractive to generative theory.

Daniels's initial proposal for a solution follows the methodology utilized in his earlier dissertation analyzing rules of Bulgarian phonology: this is to regard Russian assimilation rules in consonant

clusters as universal interpretive conventions (Chomsky and Halle's marking conventions) made context-sensitive in order to link to the phonological rules by which they would be fed. Once again, as in Cressey 1974, universal "interpretive" conventions are mysteriously converted into language-specific properties of individual grammars. Daniels recognizes, however, a paradox that "either the language particular facts are loaded into the UIC's, thus removing truly 'universal' validity from them, or the particular grammar must contain the extra information that specifies in what ways the UIC's fail to apply or are only approximated" (1973, page 368).

Again Natural Phonology rescues such a paradox in this language-particular case. The problem here would be precisely as Daniels defines it: "to let a rule of high phonetic plausibility apply to the outputs of several other ordered rules or processes without that complicating the grammar" (page 368).[14] When we recall that Stampe has defined processes as precisely these unordered rules (in the sense that they are nonlinear as established in Chapter Two) of high phonetic plausibility, we can assume such processes to be the needed type of "anywhere" rules and any ordering paradox has thus substantially evaporated. Russian consonant assimilation processes reapply each and every time their structural descriptions are met. It is the re-emergence of these Russian assimilation rules as fast speech processes which overwhelmingly establishes them as the type of innate process Natural Phonology has in view.

3.2 Naturalness and Some Notational Variants

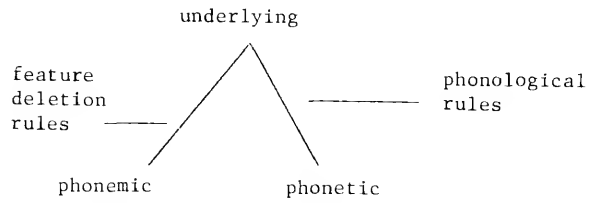
The idea of a phonemic representation being treated under the

rubric of Natural Phonology in this dissertation is quite apart from other announced attempts in the recent literature to restore phonemics within generative grammar.

Johns (1972), to cite a first example, attempts to "fit a phonemic level into a generative grammar" by suggesting these phonemic and phonetic levels be in terms of a "parallel terminal derivation" with the phonemics "not an incomplete specification of the phonetic level, but rather something that a speaker knows about [italics mine -- PCB] his sounds" (1972, page 553). Phonemic form is, adopting this first proposal, a kind of abstract "awareness" of some additional representation outside the actual steps through which the speaker carries each derivation (though not necessarily in terms of "real time" of course). Johns suggests a schematic arrangement as follows:

(11) FIGURE NINE

ABSTRACT PHONEMIC MODEL



For Schane (1971) the phonemic level is loosely "a representation of relevant surface contrasts which is deducible from the function of the rules within a generative phonology" (1971, page 503) but at the same time it is apparent in no "autonomous level within a generative phonology".

Of course for Schane this phonemic representation is in most cases "quite similar" to a supposedly phonetic output of rules in

SPE -- due, as Schane sums up, to the preoccupation of SPE with the morphophonemic rules and the neglect of rules specifying fine phonetic detail. Surface forms in SPE unwittingly approximate a Trager-Smith phonemic representation (page 520); Schane's phoneme is a formalization of this approximation by recognizing morphophonemic rules in contrast with (rather than to the exclusion of) phonetic rules, with only the former capturing the true surface contrasts. But Schane's proposal, unlike Stampe's, does not alter at all the standard view of underlying representations (see Section 3.1.2).

The attack on "phonemic competence" as a level in grammatical descriptions has its earliest and most explicit statement (as seen above in Section 2.3) from Morris Halle. The argument laid out with Halle's Sound Pattern of Russian (1959) is essentially that allowing such a level intermediate between systematic phonemic representation and phonetic representation costs a greater overall complexity in grammars, as two kinds of functionally but not always formally distinct rules will be called for -- morphophonemic and allophonic. Halle is motivated, of course, by the question: How complex does this make the grammar? Yet he effectively ignores the more perplexing question: Does the speaker show any real awareness of such intermediate representations?

Morphophonemic rules provide a level where contrast and free variation are statable and allophonic rules supposedly yield phonetic forms, yet the two classes of rules often have identical effects, and for Halle all generality in some sense is thereby lost.[15] The The shortcoming of Halle's own analysis (and indictment of Halle's version of generative theory) is the astounding degree to which this

analysis fails to account for what we do know about speakers' actual performances. It seems to make some very false predictions about how speakers essentially do assess their underlying forms. As noted elsewhere e.g. Halle's analysis of Russian leaves us with the highly uncomfortable position that the speakers of that language place no substantially different valuation on phonemic distinctions (t/d) than on corresponding allophonic ones ($\check{c}/\check{j}$).

Part Four of this dissertation is a substantial discussion of several shortcomings in one further attempt to revise an overly abstract approach to phonological forms in traditional generative grammar -- i.e. Joan Hooper's version of Vennemann's Natural Generative Phonology. Hooper's notion of "underlying representation" starkly contrasts with Stampe's, and some preliminary treatment here (the full discussion is in Chapter Eight) will again serve to further establish the uniqueness of Stampe's concept of phonemes among the alternative approaches which expound "naturalness" in phonology.

Hooper's major work (1973) on NGP establishes Vennemann's two strong conditions on phonological representation. A Strong Naturalness Condition demands that lexical forms of non-alternating morphemes remain identical to the surface phonetic forms. In frequent cases of allomorphic alternation, lexical representation is always in terms of one of the surface representations. The No Ordering Condition, in turn, requires all rules to apply in a random sequential order.

Vennemann's views of rule ordering are promising of further breakthroughs but so far unsubstantiated. It is the Strong Naturalness Condition of NGP which is most controversial, having led to

Vennemann's claim not only that word-final velar nasal in English (i.e. /siŋ/ for sing) is an underlying segment (1974a, page 216) but also that initial voiceless aspirates (/k^{h}aet/ for cat) must be after identical fashion lexicalized as well (1974b, page 347). It is an intuitive oddness about Vennemann's "phonetic" underlying representations which prompts Hooper to opt for a revised Natural Generative model featuring "archi-segmental" representations at the lexical level -- a proposal which does notably little to salvage the theoretical position of NGP. Since it appears that phonotactic constraints must apply to phonological strings consisting only of complete syllables (some evidence for this position is reviewed below in Section 4.1.1), Hooper assumes "either all rules act on lexical entries as they are stored, and they are stored as whole words (of complete syllables); or else lexical items are formatives, and no phonetically motivated rules apply until formatives are concatenated into larger strings of complete syllables" (1975, page 536), the latter position requiring a redundancy-free or archisegmental lexical representation. This seems a somewhat untenable position, however, in light of repeated earlier claims about the "concrete" approach of Natural Generative style phonology.

A substantial question facing Stampe's phonemic theory is precisely this admissibility or non-admissibility of archi-segments. The fact Hooper admits unpronounceable abstract representations as part of a speaker's competence while Stampe does not exposes contrasting commitments to maintaining an exclusively "natural" and explanatory grammar. The theory of Stampe reiterates that "systems of phonological processes are real, that the underlying and superficial repre-

sentations of utterances actually exist and are constructed and interrelated by the actual agency of these processes, and that when processes perform substitutions, these are actual substitutions occurring in the performance of utterances." Without such assumptions "the theory would be merely a 'model' furnishing not literal but only analogical explanations" (1973a, page 17). Unspecified segments are objectionable exactly because they are not pronounceable and thus never verifiable as any more than figments of the linguist's imagination. An evidence for full specification would be the speaker's recovery of neutralized forms after the fashion discussed in previous pages (cf. page 160). Also there is the substantial evidence of children's spelling choices (set forth in Stampe 1973a, pages 36-7).

Hooper (1975) raises some specific objections to Stampe's evidence against "archi-segments" which ought to be dealt with at least somewhat summarily in the conclusion to this chapter. These are criticisms in particular of the use of spelling evidence as a supporting case. Regarding e.g. the regular usage in English of the graphs p, t, k after an initial s (Stampe's partial support for the underlying voicelessness of these segments), Hooper retaliates that such line of argument is weak if only because of (1) the conventionalized nature of orthography, (2) the influence of surface phonetics on the speaker's choice of written representations at any rate, and (3) the case that some other languages (for this instance Early Modern Irish which has sgamall [skamal] "cloud" and other like forms) choose the opposite method of representing neutralized stops after /s/. Children's spellings, she admits, are somewhat better indications since they are irrelevant to the question of convention. But here the role of phonetics is

prominent according to Hooper: children spell what they hear, regardless of what they may well retain as underlying forms.[16] Hooper concludes that Stampe rejects the "indeterminacy" of archi-segments but that other linguists would adopt exactly this indeterminacy since it saves the linguist problems of arbitrary choices in positions of neutralization (Hooper 1975, page 553).

Hooper's rebuttal to Stampe conveniently avoids coming to grips with evidence that speakers regularly retain an unmarked member of an opposition when processes of neutralization have been lost (i.e. the example we have previously cited and other cases in Stampe 1968). At issue here should be what choices are evidenced in the speaker and not what choices are more convenient for the practicing phonologist doing linguistics. The crucial issue here for Natural Phonology (or for any theory of phonology) would appear to be whether or not Stampe's concrete phonemic theory will account more straightforwardly than predecessors for what speakers actually do in their utterances. It is altogether immaterial whether or not Hooper's more abstract analysis utilizing unsubtantiated archi-segments can be made to work in establishing hypotheses about natural language. (That NGP is, in fact, descriptively inadequate as well will be evidenced with Chapter Eight.) One recent proponent of more concrete analyses assumes that "it is the theory of phonology which attributes more structure to competence and the acquisition process -- imaginary representations and procedures for learning them -- on which the greatest burden of proof falls" (Crothers 1973, page 17).

The prominent question of phonology, then, is not at all how languages might conceivably be described with the aid of ingenius

and mathematically precise models (there is already a plethora of equal choices). The field abounds with a proliferation already of equally unexplanatory rational models. The central concern must be instead to construct a model which reveals the phonological knowledge of the speaker-hearer as he actually performs.[17] The strongest case for Stampean phonology to date is that it is yet to be refuted or even mildly challenged as this type of a working model.

NOTES

[1]An earlier version of some of the material in Section 3.1 of this chapter appeared first as Bjarkman 1974b. The summary of Stampean hypotheses given in this chapter is largely a review of the discussion in Stampe 1969, which remains the single available source where these claims are laid out succinctly and explicitly, with supporting examples. The particular division into five hypotheses (there is obviously a good deal of overlap) and the descriptive labels used are my own invention, and thus these should not be taken too literally as Stampean phonology.

[2]Stampe (personal communication) has gone on record as condemning Vennemann's selection of the label "natural generative" phonology, since "even at the level of universal grammar generative phonology is a descriptive rahter than an explanatory enterprise." Vennemann misses the distinction of "natural" versus "generative" as independent and even irreconcilable modes.

[3]The standard treatment of rule generalization is typically stated e.g. in Dinnsen 1974. A critique of Dinnsen's analysis of some German rules in historical perspective is given in Section 3.1.1 (page 167 and following).

[4]This list of processes is adapted from a similar list in Wojcik 1976. A fuller listing of context-free processes operating universally to govern vowel inventories is found in Donegan Miller 1972a.

[5]For information on the specific processes and their explanations the reader is directed to Donegan Miller 1972a, page 138 and following.

[6]It should be noted that Wojcik defines the scope of processes somewhat differently from the treatment here and from what I take to be Stampe's original notion. Thus: "Actually, natural phonology has no level of representation that is more abstract than 'concrete phonemic.' Rules represent relationships between concrete phonemic representations, not between concrete phonemes and some more abstract level of representation.....Rules state which phonemic representations are related to which, given the syntactic, morphological, and phonological conditioning factors. Processes are incapable of relating separate morphemes, but they do ensure that phonemic representations related by rules conform to the principle of being 'articulable' within the language" (1976, page 3). While there is nothing controversial about the claim that processes function only to govern articulation, Wojcik's denial of any level more abstract than the concrete phonemic (i.e. Stampe's natural phonemic) dismisses the line of argument in Stampe 1968 and obviates the description of Natural Phonology given in Section 3.1.2, which depends on the notion that most context-free processes and all morpho-

phonemic syntagmatic processes are prior to the phonemic level. The specific argument is carried out, as seen, in Section 3.1.2.

[7]Stampe gives a number of further examples as support, including experimentation which confirms children also pick the unmarked form in cases of neutralization. The experiment, reported in Stampe 1968, is described as follows:

> Some investigations of children's speech have turned up supporting results. One of these involved interviews with three children who regularly distinguished n from nd in their pronunciation, except before z, where they were merged by the process mentioned earlier. I presented pictures of "two [waenz]," "two [blinz]," etc., to the children and then elicited the response "one ___." The responses were invariably [waen], [blin], etc., with the unmarked [n] instead of [nd]. I found the same to be true of other morphophonemic processes (but never of allophonic ones). It appears from this that children assume that a form has the unmarked value of any phonetically neutralized feature unless they encounter contrary information (1968, page 6).

The process Stampe has "mentioned earlier" is one "which in many American dialects deleted d in 'hands' so that it rhymes with 'cans' neutralizing the opposition which appears in the singulars [haend] and [kaen]" (page 4). It could be observed, at this point, that SPE theory, as revised with the Introduction of markedness theory in Chapter Nine, would appear also to account for how speakers regularly arrive at the unmarked form in cases of neutralization, and thus would seem to obviate Stampe's attack on an earlier version of systematic phonemic analysis. But there are two debiliatating features of markedness theory which get in the way of such a solution. First, it lacks any empirical justification. Hyman, for one, observes that "to state a certain solution is superior to another on the basis of simplicity can be explanatory, if and only if the simplicity metric itself has been empirically justified. As an illustration, the failure of Halle's principle of feature counting to define natural classes has led to markedness theory, which (at least in its current state) fails in just this sense to be explanatory" (1970b, page 3). Secondly, there is the issue of explanatory adequacy. How and why do speakers acquire and implement notions of markedness values? It would be just as plausible and a good deal more explanatory should marks and marking conventions turn out to be only the manifestations of processes as Stampe contends.

[8]A summary of the relevant research and resulting positions is given in Ingram 1974:

> There is currently a controversy in child phonology regarding the relationship between the adult system and that of the child. This has been recently discussed by Kornfeld (1971), who presents two alternatives. These are:
>
> 1. The null hypothesis - "the adult system of phonological distinctions determines the child's system" (454).
> 2. The alternative hypothesis - "the child perceives and pro-

duces in his own system, which need not bear a simple relationship to that of the adult" (454).

The first of these assumes a rapid development of the perceptual discriminations necessary for the internal representation of the adult model. Studies which have made this assumption present phonological rules as a series of imperfect pronunciations. Smith (1970), for example, refers to these as "incompetence rules". The other position, however, argues that discrimination is not so rapid and that some of the errors that the child makes are due to faulty perception. In addition, this point of view, as expressed by Kornfeld, allows the child "to actively select or abstract from the adult set of features" (458).

The present paper takes the view that the child will pass through a significant part of his early phonological development with incorrect perception of the adult sound system. During this period the child's underlying system will be his mental representation of the adult model. This is similar to the position expressed in Stampe (1969), but differs in one significant way. Here, it is believed that the mental representation of the adult model is not just the result of inadequate perception, but also the result of organizational principles that the child uses to systematize this data. Specifically, it will be argued that the child represents the adult models into basic syllabic and canonical shapes upon which the above phonological rules may operate (51-2).

[9]For a seminal statement of a revised generative position on the nature of linguistic change see especially Chapter Two of Kiparsky's dissertation (Kiparsky 1965). Some apparent similarities in Kiparsky's version based on "imperfect learning" are touched on below. An important distinction between Kiparsky's theory and Stampe's, of course, is the division between rules and processes peculiar to Natural Phonology.

[10]An enkightening critique of Stampe's position on rule loss in contrast with Kiparsky's is provided by Bill Darden in his "Introduction" to Bruck et al. 1974. To quote in part:

> Halle had proposed that children automatically formulate the most economical grammar; Kiparsky proposed that if they make a mistake, it is in the direction of a more economical grammar. Kiparsky, however, could find no explanation in the theory for the most common and most natural change in grammars -- the addition of a new rule by sound change.....David Stampe addressed himself to this problem in developing his theory of natural phonology. He started with the observation that the view the phonetic processes are complexities that have to be acquired does not fit the facts of language acquisition: Many more phonetic rules than are necessary spontaneously appear in the speech of children. The unnecessary ones must be suppressed (no page numbers).

[11]It might conceivably be argued that a rule is added in this case fundamentally because this leads to greater generality and

eventually to greater overall simplicity (i.e. a devoicing rule with fewer features required in its structural description). But if the purpose of the new rule were conflation, why did this not take place immediately, rather than over the long range? And why aren't there numerous other similar additions and conflations observable in the grammar? It is just such questions as these that Stampe's view of loss as relaxation and restriction of processes seems to answer.

[12]There are specifiable reasons for the Final Devoicing process being lost from Yiddish, though there would be little motivation, as I have argued above, for the reverse interpretation of a voicing rule being added to the grammar. Stampe (1969, page 453) suggests that the cause for eventual loss of this process was not the motive of simplification but rather the problem occasioned by loss in these dialects of word-final schwas. Thousands of word-final voiced obstruents would then have appeared, lacking any morphological support and contradicting the reigning Devoicing process. Speakers (here children learning the language at the time of the loss) could elect between two possible resolutions: (1) apply Devoicing to all new final obstruents and cause wholesale mergers, like bunt and Bund neutralizing as [bõt^{h}]; or (2) suppress the process and avoid the mergers. Kiparsky 1968b contains examples which reveal that some dialects chose the second over the first option (see page 177 and following).

[13]For some discussion of this problem of repetitive levels in SPE generative grammar, see Chapter Four below.

[14]Daniels acknowledges this promising solution along the lines of Stampe's rule/process distinction, yet he gives such a possibility only brief and passing notice with his concluding paragraph.

[15]As noted above in Section 2.3, Hutchinson 1973 presents the strong argument that it is Halle's special brand of generative phonology and not Russian phonology per se that is complicated by the admission of a phonemic level and apparently necessary repetition of the voicing process. The difficulty for Halle, of course, lies in his assumption that rule applications must be strictly sequential.

[16]Of course, if children analyze newly encountered forms in anything like the fashion in which adults treat new "foreignisms" in borrowing, then they may well actually "hear" in terms of underlying rather than surface forms . I would not want to press this speculation at this point, however.

[17]Hooper makes frequent substanceless claims about the strength of NGP as an explanatory model. Consider, for example, the following series of deductions concerning perceptual rules in the adequate generative grammar:

> All these theories [Chomsky and Halle's, Vennemann's, Stampe's -- PCB] have largely ignored perceptual features -- understandably, since so little is known about perception. But there are some clear facts about perception that should not be ignored. One of

> these is that perceptual features function classificatorily. It is well known that children learn to perceive phonemic distinctions well before they learn to use the articulatory correlates of these distinctions.....Does it follow then that lexical representations are fully specified in both articulatory and perceptual features, and that perceptual rules also act as well-formedness conditions on underlying representations? In a performance model, this does not follow. It seems instead that perceptual rules should be represented in a grammar according to their function: they take an acoustical signal, and transform it into a representation that can be matched to a lexical representation. Thus their function, like that of articulatory rules, is outside the lexicon. The lexicon needs to contain only enough phonological information to classify or distinguish among contrastive segments (1975, page 559).

The first difficulty here is that there is no more proof in NGP than anywhere else about what a lexicon "needs" to contain, nor could we expect there to be in light of so little knowledge about mental representation of any type. Morphophonemic alternation gives us direct access to processes, yet we have little hope at this point to make substantive assertions about speakers' abstract representations (except what we can deduce from a backwards application of processes they apply). With its emphasis on the processes evidenced by the performances of speakers (such as in treatment of loanwords taken up in Part Three of this study), Stampe's type of Natural Phonology seems to offer far more hope for furnishing literal and not merely analogical explanations for linguistic phenomena.

CHAPTER FOUR

WORD FORMATION AND PHONOTACTICS IN NATURAL PHONOLOGY

4.1 Surface and Underlying Constraints

Since Stampe in formulating his Natural Phonology eschews any analysis of the character and role of abstract phonological rules, outside the physiologically-motivated processes (i.e. the natural processes) of the speaker's grammar, his system will encompass no distinction between purely phonological phenomena and rules which are highly morphological in origin.[1] An analysis via the mechanisms of Natural Phonology therefore corrects one imbalance in the earlier generative treatments while suspending judgment on another.

Bailey (1975), Darden (1974), Vennemann (1971b), and other rebel generative phonologists, on the other hand, maintain that one of the most evident reasons for rejecting much of the approach of Chomsky and Halle (exemplified fully in SPE) is precisely that they treat the decidedly morphological rules as though they were in no substantive way distinguishable from the phonological rules. Rules designated as morphological would be all those dependent upon references to specific morphemes, lexical items, or classes of morphemes (and therefore involving operations like those illustrated under the topic "Diacritic Features" in SPE Chapter Eight). While avoiding a number of indeterminate issues, such as that for example of "Directionality" (see Eliasson 1975), this present chapter takes up

in large part the role of morphology or "The Word-Formation Component" in the type grammar necessarily envisioned by proponents of a natural and thus explanatory system of phonology.

One issue at stake is not only the autonomous existence of a distinct class of morpho-lexical rules, but their ordering relationship to more properly phonological rules in the grammar. Anderson (1974, 1975) lists cases of numerous languages with such rules following phonological and even phonetic rules.

E.g. there is the interaction of reduplication rules which serve to establish new derivational and/or inflectional categories with the rest of the phonology: (1) Tagalog has a rule of Reduplication (of the initial CV to form intensives) following a phonological rule of Nasal Cluster Reduction (giving pamumutul "a cutting in quantity", from putul "cut" and puputul "to cut repeatedly" plus pang, the nominalizing prefix); (2) in Luiseño the fact that phonological Spirantization precedes the morpholexical rule of Reduplication accounts for a form like čukačkaš "limping" rather than an expected *čukaškaš. Also, (3) in Danish, the fact that the infinitives spille "to play" and bade "to bathe" represent exceptional forms where there is not an expected identity between the stem and related imperative form can also be explained by an assumption that a morpholexical rule (Imperative Formation) is ordered after the phonological process of Lengthening. Finally (4) there is a complex example from Rotuman where a rule for forming the "incomplete phrase" of nouns by deleting a final vowel must follow a set of phonetic detail rules which slightly raise and front vowels before syllables containing high front vowels; the evidence for this is that after the "incomplete

phrase" rule takes place the vowel of a bisyllabic word retains the vowel quality appropriate for a position before the original deleted final vowel.

Another issue is the type of formalism that most realistically expresses the apparent structural constraints operative in all natural languages. Formal constraints on morpheme structure (i.e. on permissible shapes for formatives in particular languages) have traditionally been assumed a part of the abstract phonological grammars for natural languages being proposed by generative grammarians. In the brief but tumultuous progress of generative phonology these constraints have taken on first the form of static lexical redundancy conditions (in Halle 1959, Stanley 1967, SPE) and then later the appearance of a universal set of "marking" conventions (Postal 1968, Chapter Eight; SPE, Chapter Nine). A concise but lucid short history of these developments is now provided for those unfamiliar with the issues, in the guise of Henderson's (1976) insightful discussion of lexical redundancy and low-level segmental redundancy in the SPE model. Yet even the most cursory perusal possible through such works as those Henderson in turn cites will reveal that from the outset the concept and its many applications have been fraught with almost insurmountable difficulty.[2]

Stampe (1973c) has not quite so recently called into doubt a multiplicity of claims for these "markedness" conventions with his unorthodox assumptions that such conventions are little more than accidental "appearances", however explicable, obscuring the true innate system of natural processes which govern phonological alternations for all languages. It seems instead the behavior of the

natural processes themselves that eliminates (for Stampe's analysis) the inappropriate or non-native segments and sequences of segments from individual languages.[3] Such strong objection to early markedness theory as Chomsky formulates it is not so surprising or objectionable on further reflection, once we take into account the increasing evidence for the unworkability of proposed solutions from SPE Chapter Nine (e.g. McNamer 1973; Hooper 1973, 1975; Henderson 1976 etc.).

Further, recent work on the theory of Natural Phonology, and especially Stampe 1973c, offers at least one plausible solution to the long-standing problems surrounding morpheme structure conditions (hereafter called MSCs) or redundancy rules, suggesting in effect that neither MSCs nor the universal marking conventions can claim any inherent status other than as mere formalisms of the standard theory from which they derive. A intrinsically more satisfying explanation for restrictions governing both underlying and phonetic sequences seems again to be an innate system of phonological processes: natural processes might suggest the MSCs and the marking conventions by their content, but in fact they have the same status and teleology as any other processes of the language-individual phonological component (Stampe 1973c, p. 52).

The foremost issue to be addressed here in Chapter Four, then, is the thesis that certain arguments prevalent in the current literature favoring surface phonetic constraints (SPCs) as a viable alternative to MSCs greatly distort what it seems should be the more pressing issues about the nature of phonological representations.

Clayton (1976), to cite but a single example, argues a most

uncompromising and unpalatable position: that the known SPCs are the only true generalizations about phonological segments and, to boot, that MSCs ultimately can have no place in a functionally-adequate theory of generative phonology.[4] It is my purpose to retort in the following sections that both underlying restrictions (in the form of word-formation rules and context-free "dominant" processes) and the surface phonetic restrictions (generally in the form of allophonic processes) are still valid types of strictures on phonological forms; and furthermore, I will suggest these processes (excepting the word-formation rules which are subjected to analysis in Section 4.2 and Section 4.3) are precisely the types we have labelled Stampean innate natural processes throughout Chapter Two as well as Chapter Three.

Some further difficulties attend the SPE notion of phonological constraints. Hooper and Vennemann in advocating a radical version of natural phonology (see Section 3.2 above) initiate serious additional questions about the approaches to phonotactics adopted by Chomsky in formulating generative theory. Close inspection of lexical structure seems at first blush to reveal that phonotactic constraints are manageable only in terms of conditions on syllable structure (fuller discussion is in Chapter Eight) and therefore inapplicable as kinds of well-formedness conditions on lexical items if these lexical items are formatives composed of morphemes (which are not always in terms of complete syllables) rather than of syllables.[5] Vennemann chooses a lexicon composed exclusively of words while Hooper elects lists of morphemes with only partially-specified matrices (that is to say "archi-segments") and phonetically-motivated rules applicable only outside the lexicon. While holding more to the mainstream of

physiological naturalism in phonology, Stampe is concerned only with describing the late phonetic processes and has offered preciously little about the nature of lexical representationas the necessary input to the phonological component. In general the notion of lexical representation receives but scanty treatment to date from all proponents of Natural Phonology. Nor have the generative grammarians made notably better progress, excepting perhaps the recent insights recorded in Aronoff 1976, Lightner 1975, or Halle 1973.

A second purpose of this chapter, succinctly, will be to assess some fundamental arguments raised against the possible conceptions of MSCs in generative grammar. These include here Stampe's notion that MSCs are in reality manifestations of processes, Hooper's proposal that all phonotactic constraints are word-structure constraints, and contentions by Clayton and Shibatani that surface constraints opposed to deep constraints are exclusively sufficient for capturing redundancy conditions in the phonology. Finally, I will sketch superficially in Section 4.2 and justify more thoroughly in Section 4.3 my own somewhat preliminary proposals for an improved and more comprehensive model of Natural Phonology. Given the degree to which it incorporates elements of morphology (viz. the word-formation or morpholexical rules), this model is perhaps best labelled the NATURAL PROCESS MORPHOPHONOLOGY MODEL of grammar. When more fully articulated and amended with the findings in neurolinguistics and psycholinguistics, this model should provide something approximating the necessary type of account for deep and superficial constraints on linguistic structures. That such constraints are simply not available within existing proposals for generative phonology or other brands of phonology is the special province

of discussions in the first half of this current chapter.

4.1.1 The use of morpheme structure conditions

One weakness of early generative treatments was their absolute failure to provide any explicit mechanisms in the grammar for capturing constraints on morpheme structure. And this debilitating feature comes in the face of contentions that MSCs are all but indispensible to the generative view of grammatical structures.

These earliest treatments represent, in essence, little real advancement over the Descriptivist or Structuralist approaches. In Descriptivist approaches to phonology as well as in the original distinctive feature theory of Jakobson, Fant, and Halle, nominally phonemic representations of segments were taken to contain only what was nonredundant in the corresponding phonetic representations (Stanley 1967, page 396). For early precursors of the current theory, then, phonemic representations appear as incompletely specified nonredundant versions of the phonetic representations (Henderson 1976, page 314). Filling in the blank feature values, e.g., would provide all pronounceable segments. Phonetic and phonemic forms were therefore actually manifestations of the same "level" and differed only to the degree that redundant information was not written in by the language-producing mechanism for the latter type.

It was this view of phonological levels that was attacked in Halle 1959, in the first persuasive argumentation for a level of underlying structures more abstract than those of a model fostered by Descriptivism. The level introduced by Halle was one he chose to label as "morphonemic" and one which, like the earlier phonemic

representations, left all redundant features as blanks. A difference in Halle's view of redundancy, however, was that these morphonemic representations were never simply the reflections of phonetic representations with the redundant features left unspecified. The rules which would derive the latter from the former involved feature changes of all sorts: i.e. insertions, deletions, even permutations. Halle's argument, well enough known not to demand a full restatement here, was that no level of representation related to a phonetic level merely by means of formal statements of redundancies could play any serious role in an adequately formulated generative grammar.[6]

An identifying characteristic of the phonology introduced with The Sound Pattern of Russian is a suggestion only two levels of representation have theoretical significance. Unlike subsequent full-blown generative models, Halle 1959 has no "systematic phonemic" level of fully-specified feature matrices to be distinguished from the lexical level where the redundant features have not yet been supplied. Morphonemic representations, which are the morpheme strings in their incompletely specified dictionary (lexical) shapes, are mapped directly onto pronounceable phonetic forms and this is accomplished through an ordered set of morpheme structure rules (MS Rules) followed by an ordered set of phonological rules (P Rules). Since there is no autonomous level per se of fully-specified yet still underlying forms, there is also no clear and unfailing distinction between what are to be taken as "redundancy" rules and what are the "feature-changing" or P Rules. Many apparent redundancy statements are found intermittently throughout the sets of P Rules; and MS Rules are a vaguely defined sector of the full collection of these P Rules which map an

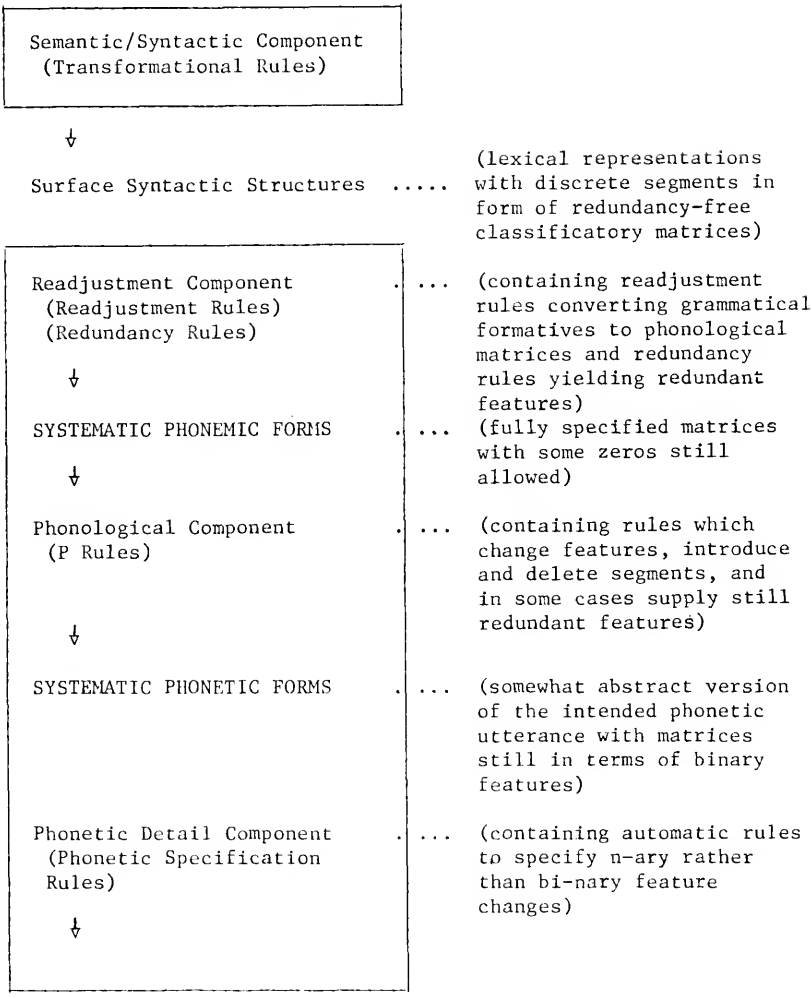
incompletely specified morphonemic level onto a fully specified phonetic level.[7]

Something of a notable advancement appears to be made with the subsequent generative models -- initially with Stanley 1967 and its proposed morpheme structure "conditions" on lexical entries and then with SPE and its revival of the Prague School notions of "markedness" -- given an assumption that it is somehow more "natural" in some never-too-clearly-delineated sense to admit two completely specified levels, i.e. phonetic and systematic phonemic forms. Stanley supposes the value in such proposals "amounts to demanding that redundancy rules be clearly distinguished from rules which change feature values" (1967, page 397). Yet here again, as with Halle 1959, any intended distinction between MS Rules and "conditions" and the bulk of the legitimate phonological rules is never made very obvious. This absence of any real empirical motivation for autonomous sets of the morpheme structure rules is a theme to which we will return regularly throughout the remainder of this chapter.

A generative grammar, as might be conceived in the pre-markedness era culminating with SPE (Chapters One through Eight), could be viewed more or less schematically, then, as having the sort of structure which is approximated in the following model.

(1) FIGURE TEN

PRE-MARKEDNESS GENERATIVE PHONOLOGICAL COMPONENT



In this tentative generative model surface syntactic structures are the output of the semantic and syntactic base components and are the strings of lexical formatives and grammatical formatives (e.g. English adjectival suffixes -ic and -al and the Tense Marker "past") which are the input to the phonological rules. But as Figure One reveals, input to the phonological rules must in fact be the matrices which are the Systematic Phonemic Representations, and these differ from lexical representations (which in turn are plugged into strings at some still-undetermined point in or at the end of the syntactic component) in so far as every formative will now have unique "phonological" matrices associated with it. The phonological matrices differ from the lexical matrices in having the redundant information of the latter (zeros as opposed to plus values and minus values) now totally written in. It is the Readjustment Component, comprising both Readjustment Rules (converting syntactic strings to appropriate phonological strings) and Redundancy Rules (filling incomplete matrices), which serve this transfer. We might note at this point that a goal of economy of representation is fundamentally involved here: the dictionary representations of morphemes need only contain plus or minus notations for just those features of pronunciation which are not predictable in terms of the general constraints on the structure of morphemes for that language. The predictable features may therefore be left blank (reducing the "cost" of the lexicon) to be filled in by MS Rules which express these constraints through the Readjustment Component.[8] One argument (not often noted elsewhere if at all the literature) that this is a demonstratably incorrect approach is based on a fact that placing zeros in the lexicon in the

very first place is dependent on some sort of knowledge of what the MSCs are to be in the given case of Language L, since the only features specifiable as zero are those which are to be filled out later by MSCs operative in the Readjustment Component. In short, then, these post-lexical constraints must somehow exist before the lexicon in any case. That such paradoxical ordering does not arise with the model suggested in later portions of this chapter is one of the strongest possible motives prompting such a revised model of phonotactics constraints.[9]

One further characteristic of the pioneer generative model is a simplicity criterion resulting in phonological rules that will also function to duplicate redundancy statements. That is, all specification of redundant information is not reserved strictly for the Readjustment Component. Given assumptions about simplicity which are endemic to generative theory, not all systematic phonemic matrices are best viewed as fully specified for all features and some matrices will therefore still carry zeros at points in the phonology. This is necessarily so since some regularities in lexical items are also observed to maintain across morpheme boundaries -- that is, they are a product of the physical conjunction of lexical items. It was at first this realization that sequential constraints must somehow reapply among the phonological rules which forced Stanley (1967, page 415 and following) ultimately to propose a convention requiring an MS Rule to reapply at any point in derivations when its structural description would again arise through the operation of a phonological rule. In one example we will call upon again below, Spanish nasals will assimilate to following consonants in place of articulation, both within and across morpheme boundaries.

Harris (1969, pages 9 and 16) cites the following examples within and across word boundary and involving nasal-obstruent sequences.[10] Word-internal assimilation: ca[mp]o (bilabial), triu[m̃f]o (labio-dental), cua[n̆t]o (dental), ca[ns]o (alveolar), ra[nč]o (palatal), and a[ŋk]a (velar). Assimilations across word boundaries: u[m#p]eso (bilabial). u[m̃#f]oco (labio-dental), u[n̆#t]io (dental), u[n#s]aco (alveolar), u[ǹ#č]arco (alveolo-palatal), u[ŋ#k]acto (velar). In light of such assimilations in a grammar of Spanish, it would seem positing an MS rule which would account for such forms through the Readjustment Component for the cases of single words, followed by an identical P rule allowed to operate subsequently on the newly-arising strings of morphemes, would constitute a rather flagrant violation of the prevelant notions of simplicity. One solution, adopted by most generative phonologists after Stanley, was to write a single P rule which would satisfy both the lexical and identical phonological constraints with a single late application. It is an example like this one of the Spanish nasals which provides one rationale for leaving unspecified the redundant information of some matrices until well into the rules of the phonological component, yet just in those cases where these rules repeat lexical constraints already spelled out for deep structures by the Readjustment Component. As we have already seen, both the Natural Phonology of Stampe and the Natural Generative Phonology of Vennemann would find vacuous the existence of any such distinctions between a set of phonetic rules and a set of lexical redundancy rules which recapitulate the same identical constraints.

The pre-markedness SPE solution to redundancy causes difficulties, then, primarily in its confused notion of the nature and location of the

redundancy statements needed by the phonological component. We find little objectionable in arguments like those of Hooper (1975, page 540) that there seem to be no empirical criteria at all by which the lexical redundancy rules might be sorted out from the phonological rules, and the few defining features that have been posited only serve to introduce further unwarranted complications for the grammar. In essence, the point is that it is exceedingly difficult to comprehend the avowed position of Chomsky and Halle in SPE (e.g. pages 171 and following), namely that in the strictest sense these lexical redundancy rules "belong to the system of readjustment rules rather than to phonology, in out terms" -- or that they affect "lexical rather than phonological representation." A safer or more reasonable assumption would appear to be that such distinctions are merely the unavoidable artifact of the particular theory Chomsky and Halle rigidly prescribe.

Chomsky and Halle do attempt to salvage the distinction "redundancy rule/phonological rule" in SPE (see page 171) by requiring that all of the so-called lexical redundancy rules always have (at minimum) two formal identifying characteristics. First, they will "fill in unspecified squares on the phonological matrices, without violating invariance" (i.e without making the feature changes that occur only with the bona fide phonological rules). Second (and a bit more puzzling in light of the apparent counterexamples provided by cases of Spanish nasal assimilation), they "apply strictly within a single lexical entry". That is to say, they are restricted, unlike P rules, to operating within sets of formative boundaries -- usually written as "+.....+". Only the truly phonological rules will apply accross these formative boundaries. But here again we encounter a proposal which can only be expected to be a

source of further difficulty, e.g. in light of the notion of collapsing MS rules and P rules in such a way that those of the latter type are permitted (as with nasal assimilations) to fill the role assigned quite intuitively to those of the former.

If the features which distinguish morpheme structure conditions (MCSs) or redundancy rules from the true phonological rules (which make actual substitutions stating relationships between morphemes) are largely obscured by the presentation in SPE, it is also true that Chomsky and Halle's position on redundancy rules itself involves some serious internal contradiction.[11] We have just established that one defining characteristic of lexical redundancy rules (and we are concerned with sequential rather than segmental redundancy here) is that they are restricted to single lexical entries. Yet in SPE Chapter Eight (page 364) the following principle of phonology is presented in relation to the formative boundary per se:

> Any rule which applies to a string of the form XYZ also applies to strings of the form X+Y+Z, XY+Z, X+YZ, where X, Y, Z stand for sequences of zero or more units and + represents formative boundary.

We deduce that Chosmky and Halle have established this convention in order to capture a certain empirical hypothesis about rules: that "processes operating within formatives normally also apply across formative boundary, whereas processes may be restricted to the position where two formatives come together". To continue with terminology we have been employing here, this convention seems to say that a process may be limited to the kind we would call a phonological rule (viz. one which affects a full string of formatives) but never to the kind we would call an MS rule (viz. one which operates only within formative boundaries). Notice that under any such hypothesis we would be left with few if any

rules that could be construed as lexical redundancy rules by the earlier definition, which restricts such rules to just that set sensitive to formative boundaries. We are now faced with another empirical hypothesis: namely, if an apparent MS rule or redundancy condition is found in a grammar to operate only within a set of formative boundaries, would not this same "rule" also apply across such boundaries whenever and if ever the possibilities for such an environment arose?

To ascertain that it indeed would, we might look no farther than the structural restrictions operative in English. The crucial case for us here (among numerous others presumably) is the English condition governing initial obstruent clusters.

Recall that English has a familiar constraint against beginning words with the cluster CC- (where C is any consonant), unless C is a liquid or the first C is s. A more restricted case of this general constraint demands that obstruents after syllable initial s will always be voiceless; we may therefore have the clusters sp-, st-, sk- but not counterparts like *sb-, *sd-, or *sg-. If we are adhering to the methodology of SPE, we may wish to capture this exceptionless surface and underlying redundancy of English through an MS rule, since it is restricted in the normal case to the single lexical formative and only to formative beginning $sC- (where $ represents syllable boundary). But natural phonologists (Stampe 1973a, page 24 and Hooper 1975, page 540) criticial of the analysis in SPE have been quick to shed light on what follows when an environment -s+C- is created by casual speech phenomena in English. An example would be the phrase let's go, which reduces to /sgo/ and then, by application of the intial-cluster constraint, to phonetic [skow]. Stampe takes this as evidence that with casual speech

those context-sensitive processes usually governing only the underlying representations may be permitted to apply to surface representation as well. At any rate, a constraint first assumed to be a lexical redundancy condition has not only applied across a syntactic boundary but also it has brought about a change in feature values - thus violating both original conditions on MS rules. Such examples argue persuasively, of course, that more than likely there are no such conditions to be captured exclusively in terms of underlying morphemes. A careful review of all rules proposed as the lexical redundancy rules may well be made to reveal that what are actually involved are in fact much more general constraints operative across the individual language, perhaps even processes which over and over again become active whenever the proper circumstances arise to motivate them.

Another facet of this English condition governing the initial obstruent clusters seems also particularly relevant to our discussion here. Theodore Lightner notes (personal communication) that this condition is found not only in the guise of an MSC but also with the phonological rule which deletes any consonant but s in word-initial pre-consonantal position. But observe the lexical item mnemonic, which is presumably underlyingly /mneymanIk/ (from the Greek source with initial cluster -- n.b. this is an analysis consistent with SPE notions of underlying representation and one which will be contradicted with the position on loanwords given in Chapter Six and Chapter Seven) but phonetically [neymanIk]. The fascinating constraint here seems to be that the MSC never applies to the Greek segment of the English lexicon (/mneymanIk/), whereas the related phonological rule applies only to Greek elements from the lexicon (giving us [neymanIk]). Such an observation, which seems valid

enough within Chomsky and Halle's own framework, will nonetheless prove sufficiently difficult for one maintaining the position that MS rules and P rules are precisely one and the same -- or worse yet, the position that all MSCs may be collapsed with the feature-changing rules as a single set within the phonology. And this, even though it provides no real evidence against Hooper's general contention that there is no sound empirical support that the types are in all cases distinct and formally separable. The strongest position we seem able to maintain is that constraints on the shapes of abstract formatives and constraints on the shapes of surface utterances are neither capable of being captured in a single set of unified processes nor separable into two definitive empirically-motivated and formally-distinct types of phonological operations.

Nasal assimilation in Spanish, as already noted, provides excellent illustration of the task involved in distinguishing lexical redundancy conditions from the actual phonological substitutions of the types known as P rules. Here we encounter a case where unwarranted complication of the grammar follows from almost any efforts in this direction.

That nasal assimilations in Spanish apply both within formatives ([bomba], [do͡nde], [em͡fermo], [ancˇo], [gaŋga]) and across some formative boundaries ([imposiƀle], [im͡fiđeliđađ], [in͡dispwesto], [insensiƀle], [ingrato]) needs no elaboration. The position of SPE (page 419) would be simply that speakers take all nasals preceding stops as the unmarked dental /n/, and that all assimilations of the above kind consequently come by way of phonological rule.

Yet again as Joan Hooper (1975, page 541) observes, first off any assumption that /n/ is the unmarked and thus underlying nasal in pre-

consonantal position for Spanish just does not hold water and the unmarked nasal in this position is always the completely assimilated nasal. And Stampe (1973a, page 50) somewhat similarly argues that a process making nasals alveolar underlyingly for English must be limited to the non-labial nasals since the nasals of lint or link or the loanword Nguyen are identified as /n/, but not that of limp, which may be represented in the lexicon with its surface nasal. For evidence that /m/ but not /ǹ/ or /ŋ/ is an underlying segment of English, one can pursue discussions at several points in Stampe's writings (but principally in Stampe 1973b, page 30). Any attempt to resolve the status of Spanish nasals on the basis of "markedness" theory would unfortunately result (since unmarked segments are assumed most natural and therefore underlying and since we are saying here that fully assimilated is Unmarked) in underlying segments like /n̲/, /m̲/, /ǹ/, or /ŋ/, which are not traditionally systematic phonemes of the language. Markedness theory at this point seems to be making a false prediction about the nature of underlying representation: its claim is that underlying representation in the case of Spanish nasals (an MSC) is a product of a universal marking convention allowing only the universally less marked alveolar articulation, while all phonetic assimilation comes by phonological process alone. This denies the seemingly very real generalization about Spanish morphemes that all nasals are assimilated, which is as much a restriction on the Spanish language as is the initial cluster constraint (syllable-initial /s/ is always followed by a voiceless rather than voiced obstruent both lexically and phonetically) in English. No Spanish speaker is about to accept anything but [iŋgrato] with its fully assimilated nasal as well-formed. (N.b. by contrast that English speakers accept

both [k^haet] and [kaet], where the aspiration process is pure allophonics and does not imply anything about the nature of the lexicon.) Hooper suggests a possible alternative approach is to assume that all rules referring to syllable structure are "phonotactic" rules and must alone apply within the lexicon (MSCs). But again, this same nasal assimilation rule is prohibitive of such a theory since the rule has clearly to do with Spanish syllable structure (Hooper 1972) and is thus by definition "phonotactic", but yet it is also required to apply to numerous strings outside the lexicon. The single conclusion Hooper sees as supportable is that these attempts to separate phonotactic rules into distinct sets applicable at different levels of representation can only lead, in our present state of misunderstanding, to just such unmanageable inconsistency and complexity throughout the grammar. (The inconsistency and complexity dissipates promptly, however, when we assume later in Section 4.2 that both lexical restrictions and pronounceability are together established by a plethora of innate processes which render quite vacuous the need for distinctions like redundancy rules versus P rules as well as the separate notions of a Markedness component.)

One final difficulty in standard generative theory associated with MSCs is a highly questionable assumption that a lexicon of a natural language consists of a listing of morphemes and (concomitantly) that constraints on phonological shape are thus the constraints on morpheme structure. Both Vennemann (1974b) and Hooper (1975) have been to some degree convincing, on the other hand, in their arguments that a lexicon is better perceived as a listing of entire words and not of morphemes per se. Or, at least that the lexical "Dictionary" to which constraints on phonological representation as well as actual phonologi-

cal substitutions apply is made up of entire words, and constraints in phonotactics are conditions which refer to word-structure and syllable-structure rather than morpheme-structure as heretofore blithely assumed.

The evidence that phonotactic constraints are not to be stated in terms of morphemes is largely of a negative kind. We can review here very briefly five illustrative examples discussed at much greater length elsewhere by Hooper (1975, pages 536-39) and perhaps only representative of additional similar arguments which might be summoned forth.

(a) The intractable nature of the "morpheme" with regards to any set of "phonotactic" constraints is not at all puzzling when we recall that the morpheme is not at all a phonological unit by design but rather a syntactic-semantic unit. It is foremost a unit of meaning and only incidently a unit of sound. In fact, some grammatical morphemes have no phonological shape at all (example: the past tense morphemes in the fossilized English verbs like hit); some grammatical morphemes can not be separated from lexical morphemes which encompass them (example: the past tense morphemes in English strong verbs like sang); and some other grammatical morphemes have a phonological shape which although real enough is incapable of standing independently (example: the past tense morphemes of English regular verbs like smoked [t] or blowed [d]).

(b) Among the lexical morphemes, it is usually possible to state clearly the phonotactic constraints operative on the beginnings of stems but not for the endings of morphemes. Of course this is the case precisely because stem-initial always means syllable-initial as well. In verb stems which take suffixes stem-final might well mean syllable-final, syllable-initial, or even neither. We encounter such stems in Spanish as abl- (hablar), kompr- (comprar), or sent- (sentar). For Spanish, to

take this single example from Hooper, there are numerous possible stem-final clusters yet only two (ns, rs) syllable-final ones, as well as considerable constraints on single consonants occurring syllable-finally. An inescapable conclusion is that there are considerable generalizations possible about syllable structure for Spanish (see Chapter Eight below) yet almost none of these are relevant if one persists in talking about morpheme structure.

(c) The economy generated by syllable-based constraints is far greater than that for languages with morpheme-based constraints. This is illustrated by the straightforward fact that statements of distribution for the segment within the morpheme are necessarily more complex. Hooper gives examples with the Spanish affricate /č/ (examples: chino, ancho, bolchevique, marchar, macho) which always occurs only at the onset of the syllable. No such generalization is available when reference is made to the structure of the morpheme. With the Spanish words just listed we would be left, in the case of the morpheme environment, with such specifications as (1) after morpheme boundary, (2) after nasals, (3) after liquids, (4) after vowels, and the like.

(d) If one were to attempt formulating MSCs for Spanish (say, in addition to syllable-structure constraints), then he would have to also distinguish formally between stems and suffixes. An acceptable ending for a stem (example: mpr for kompr-) will not necessarily be one for a suffix, and vice versa.

(e) The psychological reality of MSCs is to be seriously questioned as well, since it never has been established that these conditions will capture a speaker's intuitions about what are acceptable morphemes of his language. Here we need look at only two pieces of compelling evi-

dence. Most examples in the literature of the speaker's intuitions about acceptability are examples of impossible syllables (Hooper's example: the English */bnik/ versus /ab$nə/ Abner). Also, though a form like /-ndo-/ (the Spanish progressive morpheme)is perfectly acceptable as a morpheme, speakers dont seem to be inclined to identify it as a permissible sequence in Spanish (Hooper 1975, page 538). This is perhaps the most compelling example of all, then, that it is the syllable and not the morpheme of which the speaker is most directly (perhaps exclusively) conscious.

4.1.2 The use of surface phonetic constraints

The question of morpheme structure conditions seemed to be once and for all resolved in the immediate wake of Stanley's (1967) monumental paper. Stanley, like Halle and some of the Structuralists, adopted the morpheme structure "rule" as both a constraint on phoneme sequences and an algorithm which would predict redundant feature values in the phoneme sequences. But Stanley's view of redundancy rules departs from the previous treatments when he makes the following series of related assumptions:

(a) that redundancy rules are included in the MS rules in such a way that their output (i.e. the input to the P rules) is fully specified matrices (that is, they function to produce the "systematic phonemic" level of representation);

(b) that MS rules are redundancy rules only and therefore never alter feature values;

(c) that MS rules are always an unordered set of applications, and in this sense again they are distinguished from P rules;

(d) finally, that the most natural way of formulating the MS statements is not as "rules" at all but as a static set of conditions on formative

shapes all applying at once to the same level of representation -- the underlying lexical forms.

Of course this approach, in essence a model for the pre-Chapter Nine version of SPE, exhibits all the same shortcomings discussed so far in this chapter involving a true distinction between any such "conditions" and the subsequent phonological rules, which may often repeat their function as simply feature-filling devices.

The introduction of "markedness" conventions in Chapter Nine of SPE was the generative phonologist's response to the need for supplying naturalness and universality to the idea of formal restrictions on lexical representation.[12] A single dictionary level of formatives with (+) and (-) features, as well as redundant (0) features, is replaced in the markedness version with distinct levels of representation. The more abstract level has all features valued as either M(arked) or else U(nmarked) which represents "normal" and "not normal" and is based on a whole range of assumptions about the naturalness and universality of particular features. Among the stops, for example, p/t/k can be assumed more natural a series than p/t/c (c = voiceless palatal stop). Or among the affricates, the palato-alveolar is the less rare or less "marked". A less abstract level of the dictionary has every cell specified as (+) or (-) following the application of the marking rules or "conventions" which have now been substituted for MS conditions as the feature-filling device. But the crucial assumption about the new markedness approach of Chomsky and Halle was that the less abstract (fully specified) level of the dictionary was a product of universal rules and that in every context some feature value is "natural" and thus costless while the other is "unnatural" and therefore costly (see

the specific example in regards to Spanish glides discussed above beginning on page 188 in Chapter Three). In other words, the Marked-Unmarked notions of SPE Chapter Nine (along with a more expansive version found in Chapter Eight of Postal 1968) involve a claim that behind the particular phonological systems of individual languages lies a truly universal structure. Articulatory and perceptual factors underlie the concepts of Marked and Unmarked. The implication, then, is inescapably that very few language-particular conditions are actually needed to account for morpheme structure "constraints", since most of these constraints are actually universal. The essential misdirection in this markedness notion (which postulates static conditions rather than active processes or substitutions) will become hopefully more apparent when we take up features of a model of Natural Phonology in the paragraphs below.

There seem to be essentially two things wrong with the MSCs as these were envisioned initially by Stanley -- two features which strained their credibility. One was the lack of any empirical evidence for a distinction between MS rules and P rules which we have taken up above. The second is the seeming "unreality" of a set of static conventions governing redundancy which would appear to have little observable relationship to the detectable behavior of the individual speaker. Stampe (1973c, page 49) cites an advantage of SPE Chapter Nine to be precisely its return to some degree of insight at least (in so far as conventions are more process-like than redundancy rules and conditions) that restrictions on underlying representation are due to processes and not to mere frozen conditions. Stampe would like to assume that a logical next step after SPE would be our further apprehension that conventions are in fact what the natural phonologist has been calling

all along the "natural" processes or "natural" rules. But I will delay further presentation along these lines until the presentation of an alternative model for phonology in Section 4.2.

Another weakness of Stanley's approach to phonotactic constraints is not quite so readily met by improvements through markedness theory. This is a failure to account for the possibility that MS rules and redundancy rules may also have an application at the phonetic level as well as at the lexical level of representation. In this light, the following exerpts from Stanley's paper (1967, page 397) are highly suggestive:

> It is possible that, just as MS rules state redundancies at the systematic phonemic level, there are rules which state redundancies at the systematic phonetic level. In fact, we have seen that it was just such a statement of redundancies at the phonetic level which was regarded by many linguists as, essentially, constituting a phonemic system. It is thus possible that stating phonetic redundancies in this way will enable us to capture something resembling a phonemic system, even though we are working in grammars which have only two levels of representation, systematic phonetic and systematic phonemic, neither of which resembles a phonemic level.....Phonetic redundancy rules would enable us to simplify the late P rules, for these P rules would have to state only the non-redundant, never the redundant, features of the phonetic level. Further, they would provide a framework for making generalizations about the phonetic pattern of a language, a framework which is, at present, conspicuously lacking (my italics -- PCB).

But this is as far as Stanley allows himself to speculate. There remains no mechanism within generative grammar (before the work which was to emerge as Shibatani 1973 and which constitutes more of a recognition than a solution for the problem) for capturing generalizations about phonetic patterns in the same sense that we can capture those about the morphophonemes of a language.

Recent papers by Shibatani (1973) and Clayton (1973, 1976) have now argued compellingly as well that generalizations about phonetic patterns indeed a necessary part of any psychologically real grammar. Shibatani,

in particular, attempts to demonstrate that aspects of linguistic competence once attributed to morpheme structure conditions are more properly a manifestation of surface phonetic constraints, two examples perhaps being the speaker's intuitions about "nativeness" in nonsense forms and about the "admissibility" of foreign borrowings. Shibatani suggests, for example, that where a German rule of Final Devoicing fails to apply to underlying strings like /bũnd/ and /rãd/, allowing these systematic phonemic forms to surface, speakers will reject phonetic [bund] and [rãd] as being admissible forms, since the full application of obligatory P rules (giving the expected phonetic form) has not yet been carried out. For Shibatani, a grammar must be composed, then, of a complexity of partially overlapping sets of morpheme structure conditions (MSCs), surface phonetic conditions (SPCs), and obligatory and optional phonological rules. It is possible to discuss apparent constraints of a number of kinds, which are given labels like those below by Shibatani.

(2) FIGURE ELEVEN

SHIBATANI'S SURFACE AND UNDERLYING CONSTRAINTS

UNDERLYING ONLY

MSC: A morpheme structure condition which is not a phonetic constraint and which need not be represented by any phonological rule.

*A/MSC: A logically possible category not mentioned by Shibatani but discussed in Clayton 1973 -- this would also be a morpheme structure condition which is not a phonetic constraint but which must be represented by a phonological rule (and which most likely is therefore non-existent).

UNDERLYING AND SURFACE LEVELS

M/SPC: A morpheme structure condition which is also a phonetic constraint, but which again need not be represented by any phonological rule.

A/M/SPC: A morpheme structure condition which is also a phonetic constraint, and which must in addition be represented by some phonological rule.

SURFACE ONLY

SPC: A phonetic constraint which is not a morpheme structure condition and which must in turn be represented by a phonological rule.

Clayton (1973, 1976) replaces Shibatani's assumptions about the necessity of limited mechanisms for stating surface as well as underlying constraints with the much stronger claim that surface phonetic constraints are themselves entirely sufficient for a grammar (that is, they are the only true generalizations either possible or necessary) and that MSCs thus play no independent role and have no justifiable position in any form of modern phonological theory. Clayton's strong claims against the reality of MSCs are based in large part on Shibatani's earlier definitions (Figure Two) of types among phonetic and phonological constraints and are supported with examples from synchronic and diachronic facts in Spanish and Latin. Her examples, subtle and often intricate, are in large part beyond the scope of this chapter. I will repeat here only the general thrust of her conclusions, and these take essentially two related forms.

(a) First, MSCs appear to have no empirical support since they are neither true at the surface level, having no corresponding SPCs (direct evidence), nor work to bring about any alternations, not being represented by any phonological rule (indirect evidence). As a single example, Spanish has a segmental constraint requiring that /f, s, x/ are the only available underlying continuant obstruents (in standard dialects); yet [z] appears also in most of these dialects at the surface level (e.g. in de[z]de or mi[z]mo) and no synchronic rule operates to devoice the surface obstruents (thus restating the constraint at the surface level).

(b) Then, if MSCs may be rejected due to absence of any empirical support, all remaining underlying generalizations (M/SPCs and A/M/SPCs) are the subset of a larger set of surface generalizations (M/SPCs, SPCs, A/M/SPCs, and A/SPCs). Since there do exist SPCs which do not correspond to MSCs (n.b. Shibatani's evidence), we can assume that some

information required by the grammar is provided by SPCs which can not be provided by MSCs. To demonstrate that both types of constraints are in fact necessary it would be essential to demonstrate first off that MSCs provide information not available through SPCs. But apparently no such information is ever available exclusively through MSCs.

The relevant fact about Clayton's treatment does not seem to be so much whether or not it handles adequately the arguments and data arising from Latin and Spanish but rather whether or not it has ignored some pertinent cases in the literature where the operating phonetic constraints are ones which seem to play no really essential role. Cases of this type may be found intermittently among the work on loan phonology and I will choose here to review just a single example from Kaye 1973. The point of this particular example, in essence, is that although MSCs may not be true at the surface level, nor function to bring about any productive alternations, they do seem in isolated cases to have a good deal to do with how speakers adopt foreign borrowings. Although these apparent MSCs may prove on closer inspections not to be conditions on "morpheme" structure as such, it is at the same time indisputable that they are not surface conditions. Clayton's account of the synchronic grammar rules out any explanation of this type of phenomena, however, based as it is on the assumption that MSCs never provide any information to the speaker not also available through extant SPCs as well.

Odawa (Kaye 1973), a Canadian dialect of Ojibwa, displays a MSC which provides that no major category morpheme may initiate a consonant cluster. That this is not a surface level "phonetic" constraint but a legitimate MSC (i.e. an underlying not superficial restriction) is affirmed in so far as surface level initial clusters arise from a rule which deletes

unstressed vowels. The examples we are interested in here involve the cases where Odawa has borrowed English words having initial voiceless obstruents. The English voiceless obstruents are treated as fortis consonants by Odawa phonology, and these fortis consonants are underlying clusters and therefore obey the relevant MSC which blocks their occurrence in itial position. An English word like pen would apparently be borrowed by Odawa as underlying /ppēn/ with the initial CC representing a fortis consonant. Here a possessive form /ntappēn/ (for "my pen") suggests an actual underlying shape like /appēn/ (all intervening rules are quite irrelevant here and will be consistently ignored). If /ppēn/ had in fact actually been borrowed as the underlying shape, contrary to the operating MSC, then the relevant rules of Odawa would have provided a possessive in the shape of */mppēn/, a form which in itself violates no Odawa phonetic constraints. It is also worth noting that when Odawa borrows from English any word with initial voiced obstruent no initial penthetic vowel will appear in underlying representation. The English word bus is borrowed phonemically as /pass/ with the possessive form being /mpass/. The only explanation we seem to have, then, for the initial vowel of /appēn/ is the requirement that the loanword conform to the underlying constraint against initial clustering. Thus we have here one forceful case at least where a constraint on abstract rather than phonetic form seems to provide the true and appropriate generalization governing restrictions on grammatical form.

That this restriction on CC clusters in the lexicon of Odawa has no surface reflex or is repeated in no productive phonological rule seems to make it no less a restriction on the linguistic behavior of the Odawa speaker borrowing English forms. In subsequent sections further

examples of this kind from loanword phonology will provide additional evidence against Clayton's inappropriate claim that SPCs are the only generalizations to be captured by an adequate theory of grammar. First, however, it is appropriate to take a closer look at the type of phonological model to be assumed as basic throughout the remaining chapters.

4.2 Active and Passive Grammatical Components

We have already established that a standard treatment of morpheme structure constraints is insufficient and often leads to inaccurate claims about the grammars of individual speakers. Also, we have noted that surface phonetic constraints, though an attested phenomenon in grammars, themselves are not sufficient to account for all phonotactic constraints. The Odawa loanword example suggests that certain language-peculiar constraints are of an underlying and not a surface nature.

Stampe (1973c) has also presented forceful arguments that markedness theory of SPE -- the formal replacement for earlier MS conditions -- has apparently little to do with our actual identifications of underlying segments and that Marking Conventions therefore have no real role in actual phonological systems.

In Stampe's Natural Phonology there are neither "implicational laws" nor "markedness conventions" but only an innate system of phonological processes which may indeed lend the appearance of such implicational laws and markedness conventions but which in reality has the identical status of the processes ("rules") in any language-particular phonological system (Stampe 1973c, page 45). What remains essentially accurate about the markedness theory of phonological constraints is the original contention that all such constraints derive ultimately from the universal principles which are inherent restrictions on perception and articulation.

What is misleading is an assumption that such proposed conventions have any independent status outside of the phonological processes which they ultimately reflect.

In the Natural Phonology propounded by Stampe, then, markedness is a result and not a cause of the nature and application of universal processes. Patricia Donegan Miller's work on vowel systems, as an example, reveals that processes operating language-specifically produce not only the expected triangular vowel systems but also the unexpected and even surprising systems like that of Icelandic.

(3) ICELANDIC VOWEL SYSTEM

i	y	
e	ø	ɔ
	a	

When Icelandic u, o converts to y, ø, we have "marked" values displacing the "unmarked" values and the latter are never subsequently replace by those of less-marked status. In support of an argument that markedness does not affect identification of underlying segments, Stampe observes (1973c, page 51) that when a child inserts schwa between initial /s/ and a following consonant in English and removes /s/ in the process from its original preconsonantal environment, the /s/ does not become an "appropriate" lesser marked consonant like /p/ or /t/. Yet such an event might be expected if "least marked consonant" had some sort of completely independent reality.

If original MS theory and subsequent markedness theory are equally inadequate and inappropriate for handling the problem of phonotactic constraints, it is at the same time obvious from all descriptions of individual languages utilizing the generative method that such formal

constraints themselves are real enough and demand inclusion within any reasonably complete model. What is seemingly needed is a mechanism which accounts for three apparent kinds of genuine "filters" on phonological structure in all languages. First, there is a necessary segment constraint which limits what segments are available from a given lexicon. English, we will assume, has no underlying nasal vowels, for example, and no underlying ʔ (see Section 2.3), while Spanish can be taken to have no underlying z. Second is a sequential constraints mechanism governing in some arbitrary or perhaps more natural fashion what strings may occur underlyingly, whether or not these combinations may be also maintained phonetically. And finally, we must allow for a system of surface sequential constraints of the nature of SPCs which account for what strings may actually surface in the phonetic representations of the individual language.

Inevitably the problem inherent in envisioning such a model is in essence the problem of determining relevant phonological levels. The issue at the heart of current phonology is that of arriving at some proper "stratification" of the grammar which might establish the levels where phonological representation is actually and measurably constrained. The difficulties encountered when treating phonotactics in generative grammar seem to fall exactly here, in the imposition of a limitation of two relevant levels of representation. Relevant in generative theory are only the phonetic level and the systematic phonemic level; for generative phonology, then, the only significant distinction is the one between redundancy rules (later markedness conventions) and phonological rules, with only the latter performing actual substitutions. It is only the redundancy rules or markedness conventions, then, which

can actually constrain phonological representation -- a fact which has inter alia left us with no appropriate mechanisms for handling surface level constraints. And as already seen, there is at any rate no empirical motivation for such a division into MS rules and P rules in the first place. This division is, of course, the single overriding cause of so much confusion in the generative camp over the issue of capturing proper restrictions on underlying forms.

We now arrive at an essential uniqueness in a posited model of Natural Phonology as opposed to a standard model of generative phonology. Whereas in the latter it is only the markedness conventions or redundancy rules which constrain phonological representation, for the former this function is served by the innate processes which are foremost the constraints on the articulation of phonetic structures as well. Whereas "rules" in Natural Phonology capture the phonemic relationships that hold between the individual lexical items of a grammar (Wojcik 1975 and Chapter Two above), processes take on the combined role of phonological rule, morpheme structure constraint, markedness convention, and surface structure constraint. Processes come in essentially two distinct kinds: there are perceptually-motivated processes such as diphthongization, which apply context-free, intensify the properties of individual segments, and work to neutralize underlying distinctions through unselective elimination of unpreferred segments (i.e. they establish the underlying inventory of segments); and additionally there are context-sensitive processes such as assimilations, which function to facilitate articulation (i.e. they provide the surface phonetic restrictions). Processes, then, have the dual purpose of facilitating articulation (syntagmatic ones) and facilitating perception (paradigmatic ones). And phonemic

or underlying forms in Natural Phonology are comprised of just those segments which are not banned from the language by paradigmatic processes and at the same time not predictable by the operation of the purely allophonic syntagmatic processes.

The primary advantage of Natural Phonology would seem to be its assumption that all posited levels of representation are mentalistically or physiologically real and that all substitutions are likewise actually made in the course of a derivation. The evidence for this assumption is elaborated throughout Stampe's works (1968, 1969, 1972, 1973a, 1973b, 1973c) and need not be repeated intact here. A second considerable advantage, and one more basic to our discussion here, is an assumption of at least three distinct levels of phonological representation. The phonetic representation in Natural Phonology takes on an added importance as a motivation for more abstract forms (such forms being always recoverable by retracing the sequence of processes that have applied, a type of analysis which speakers apparently actually make in assessing loanword data). Abstract representations in Natural Phonology generally correspond directly to a phonemic "level" rejected by generative theory. To repeat a notion introduced earlier, the child is assumed to adopt an initial underlying phonological system which approximates the phonemic system of the adult speech around him, and he is likewise assumed to postulate more abstract forms only when these might be needed to account for troublesome phonological alternations. Stampe's view of underlying form approximates Sapir's or Bloomfield's, except that where some natural process has neutralized distinct lexical features (as with the German final obstruent devoicing) the speaker must have an access to morphophonemes in the individual morphemes that require them. Forms

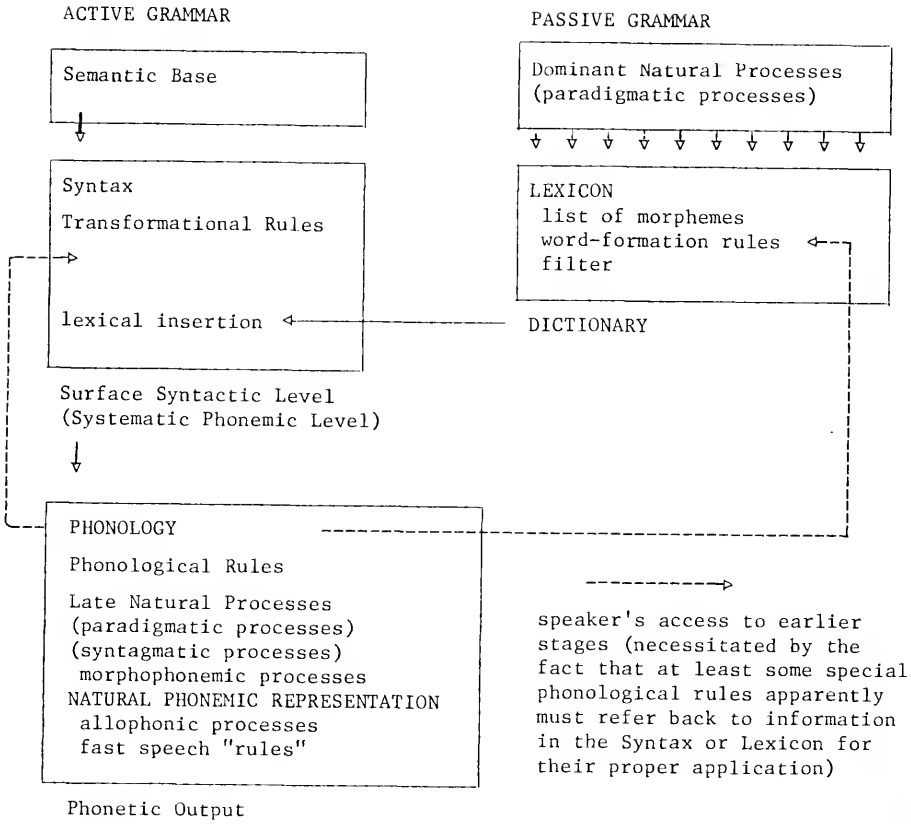
without these alternations retain shallow phonemic representations and even in an adult grammar the underlying forms will be expected to largely correspond to a traditional "phonemic" level. The learned rules and the morphophonemic processes (i.e. those processes which introduce segments not barred from underlying representation and which thus often neutralize underlying distinctions), which together establish the shallow phonemic representation, are applied to formatives from the lexicon. The learned Velar Softening rule of English, e.g., converts the original systematic phonemic or lexical stem /il&ktrIk-/ into the new phonemic stem /il&ktrIs-/. Here we have a third level which may correspond to roughly the "surface structure" or systematic phonemic representation in generative grammar (viz. lexical forms after all matrices are specified). Yet it is not this deeper "underlying" form but rather the more shallow phonemic level to which Stampe refers when claiming that the child "lexicalizes" the phonemic forms of the adult or that speakers of the target language "lexicalize" the surface utterances heard from the source language in borrowing. In full derivations, rules and morphophonemic processes apply to lexical representation yielding the phonemic output. Late syntagmatic ("allophonic") processes apply to the phonemic form yielding a phonetic output. Phonetic output as it is heard from other speakers in turn exerts influence directly on what may become underlying forms in the child's first acquisition of language or in the adult speaker's manipulations of perceived lexical shapes for loanwords.

This view of a grammar makes possible solutions to a number of the issues of general phonotactic constraints discussed in earlier sections of this chapter. Let me first sketch here a model which may be referred to as a Natural Process Morphophonology and which will

serve as a pictorial representation of the type of Natural Phonology we will have in view throughout the remainder of this study.

(4) FIGURE TWELVE

NATURAL PROCESS MORPHOPHONOLOGY MODEL



SYSTEM OF NATURAL FILTERS ON PHONOLOGICAL STRUCTURES

FILTER ONE

Dominant Natural Processes act as a filter on segments admissible in the Lexicon (e.g. the process that will denasalize all vowels underlyingly for English). (Stampe 1973a)

FILTER TWO

The Lexical Filter specifies idiosyncratic properties of individual words and yields a set of actual words in languages from the set of potential words (established by lists of morphemes and the word-formation rules; e.g. the English potential words like *derival or *arrivation are formed by word-formation rules but marked as

FIGURE THREE CONTINUED

[-lexical insertion] by the lexical filter). (Halle 1973 and Botha 1974).

FILTER THREE

The Phonological Component contains both acquired and primitive articulatory processes (i.e. rules and processes) which have the effect of imposing a phonotactic filter on all speech outputs. Language-specific selection of processes acts as the "surface phonetic constraints". (Stampe 1973a and Braine 1974).

One division of this proposed model is between what are termed the Active and the Passive grammatical components and this distinction must first be clarified. Halle (1973) has pioneered in justifying a morphological component for generative grammar which would be comprised of a list of morphemes (all the possible occurring morphemes of the language), rules of word-formation, a filter eliminating words produced by the word-formation rules but nonetheless non-occurring, and a resulting dictionary of items available for lexical insertion at some point in the syntactic component (cf. also Harris 1974 for examples in Spanish and Aronoff 1976 for more elaborate theoretical treatment of Halle's model). It is essentially this conception of a morphological component, adjusted to account for the notion of paradigmatic processes, which I would adopt for my revised model of Natural Phonology.

As Halle emphasizes, the word-formation component and it resulting dictionary of lexical items are likely stored in the speaker's permanent memory and invoked only when he must treat new and unfamiliar words (viz. in language acquisition and loanword adaptation). In each instance of deriving an utterance containing the English word arrival, for example, this item would not result from fresh application of word-formation rules but instead be selected in its ready-made form from the existing list of dictionary items. In exactly this same sense, I would propose that "dominant" paradigmatic processes which Stampe proposes are a filter on lexical representations (e.g. the process which eliminates nasal vowels from the English lexicon) are passively stored (i.e. are a feature of linguistic "competence") any only applied when prohibited segments are encountered, say in borrowing. Again, in the derivation of each and every sentence the denasalization rpocess is not applied. Of course,

one of the features distinguishing child language is precisely that such paradigmatic processes often do have much wider application; thus, in an example to which I return below, children often generally apply a process deleting glottal stops which thus becomes a restriction on surface utterances as well as lexical representation.

The first proposed "filter" or constraint on phonetic structures in my revised model (one which corresponds to the MS rules of standard theory) is a dual filter supplied by this Passive component of the grammar. The dominant paradigmatic processes, in so far as they remove impermissible segments from the lexicon, provide all necessary segmental constraints within the grammar. Underlying sequential constraints on lexical shapes are those achieved through the existing list of morphemes in combination with the word-formation rules and special lexical filter proposed by Halle. Together, the paradigmatic processes (excepting those rare late applications affecting vowels of a type discussed in Stampe 1972) and the several components of the "morphology" establish what segments and sequences of segments are available for lexical insertion and subsequent treatment by the phonological rules.

A final "filter" or restriction on what are acceptable surface utterances is supplied by the late syntagmatic (allophonic) natural processes which apply at the extreme end of the phonology. Again, such processes are well-defined and motivated in Stampe's work and comprise much of what are traditionally labelled the phonological rules of the grammar. A full distinction between "processes" and "rules" has already been provided above in Chapter Two. The applications of such processes whenever their environments are met (a feature of processes) fully eliminates, as far as I can determine at present, any need for stating additional mechanisms

such as "surface phonetic" constraints. If we recall an example from Shibatani mentioned earlier, where speakers of German appear to reject forms where the normal process of devoicing has not applied (i.e. where these speakers apparently yield to a "surface phonetic" constraint which requires devoiced final obstruents), it is now evident that such a constraint is in reality the manifestation of a predominant syntagmatic process applicable for German. As another example, we can recall here also Hooper's observation that the apparent MS condition of English blocking voiced obstruents after initial /s/ appears under another guise in the fast speech rules (late syntagmatic processes) of English in a phrase like [skow] "Let's go". The explanation would seem to be that it is a natural process -- an actual substitution motivated by ease of articulation or other conditions on naturalness -- and not a static convention which is in all cases responsible for the voicelessness of the consonants which follow the English initial /s/.

I wish now to turn to one final example that restrictions on underlying as well as surface shapes is a result of innate processes or actual substitutions and not static conditions on lexical forms. This example is again one already mentioned by Stampe (1973c, page 48). Stampe observes that generally the research on the acquisition of phonology will verify to a large extent that conditions on underlying shapes of formatives are originally innate processes, which in the child's speech will cause actual substitutions but in the adult grammar are reordered to a point in the grammar where they may serve only as restrictions on lexical representation. As mentioned above, children often delete glottal stops from English words ([bʌʔn] becomes [bʌn] "button"). Stampe's conclusion from this example (and I take it to be a reasonable one) is that when

these glottals appear in the speech of the child it is due to the fact that he has now ordered (but not deleted) the process before that which substitutes glottal for t. It is precisely this same process which now accounts for why in the adult grammar all glottal stops which can not be analyzed as derived from /t/, say when treating foreign borrowings, are necessarily deleted. Stampe notes that the treatment of this restriction in standard theory as a redundancy rule would have to predict that foreign glottal stops should be borrowed into English as [h] (that is, assuming a redundancy rule which reads ʔ -> h). But of course this is not at all what happens.

4.3 On the Reality of Morpholexical Rules

It remains to provide some evidence for Halle's notion of a word-formation component and one confirming case immediately presents itself from the treatment of Mandarin Chinese in Cheng 1973 (see also discussion in Ch'u et al. 1975). It is Cheng's analysis of Neutral Tone in Mandarin which not only sheds light on a long-misunderstood aspect of Chinese phonology but also lends support to the model tentatively announced in Halle 1973. In essence Halle's proposal, aimed at a long-acknowledged gap in a recognized standard generative model, is that the speaker's lexicon results from an active morphology comprised of a stored list of all potential morphemes of the language, a large set of word-formation rules or templates stating combinatorial restrictions in the possible form of (5) through (7):

HALLE'S WORD-FORMATION RULES FOR ENGLISH

(5) $[\text{STEM} + \text{i} + \text{ity}]_{\text{n}}$

(6) $[\text{STEM} + \text{al}]_{\text{adj}}$

(7) $[\text{verb} + \text{al}]_{\text{n}}$

and a lexical exception filter to assure removal of all well-structured but non-occurring items as well as to specify all semantic and phonological idiosyncracies peculiar to certain exceptional forms. E.g. English nouns like transmittal and recital have special features of meaning not predictable from their verbal sources, while entirety and nicety are two cases which for some reason do not undergo the familiar English Trisyllabic Shortening rule which is exemplified with similar forms like serenity and obscenity (for details see Halle 1973 and Botha 1974). Essentially the point to be made here is that in the absence of any persuasive alternative analysis it is precisely evidence like that surrounding Cheng's treatment of Mandarin nuetral tone that best recommends Halle's notion of such an exception filter as a vital additional component of generative theory.

It is the existence of semantically distinct pairs like [sə̄ŋ xuó] "life" and [sə̄ŋ xuo] "livelihood" which makes it altogether evident that Mandarin has a fifth category of tone which is at least phonetically distinct from four basic or underlying tones. Furthermore, the evidence is considerable for saying that this neutral tone, perceived as a short and lax syllable, arises from two distinctly unrelated yet recoverable sources. It is firstoff a commonplace of Mandarin phonology, note, that stressed syllables display the four basic tones of Mandarin and that when a syllable is for any reason unstressed its tone in turn becomes neutral. Cheng's assumption is, then, that all syllables displaying neutral tone must be identifiable with one of the basic tones in abstract representation, and it is the conditions of surface structure which determine removal of (or at least non-assignment of) stress and thus the characteristic of neutral tone.

One example Cheng cites is the phrase [tá ni] "to hit you" (page 64). In this verb-pronoun structure the pronoun is unstressed by syntactic rule and consequently assigned neutral tone. That a neutral tone is not basic here is latogether apparent. The lexical item nǐ appears with third tone in normal cases where it is stressed; and further, the word tǎ is also normally in third tone but has quite evidently been converted here to second tone by operation of the independently well-motivated Tone Sandhi rule applicable (in this case at some abstract level) whenever we have two adjacent third tone syllables.

Syntactic rules are similarly responsible for the nonstress and related neutral tone of at least all the following additional items in Mandarin: particles; suffixes; localizers; directional verbs; reduplicated morphemes other than adjectives, numerals, or classifiers; and resultative verb endings (pages 58-59). Yet we have already noted that neutral tone apparently has defensibly two distinct sources and in addition to this syntactic conditioning there are as well alternations like [ɕüəŋ tì] "brothers" with [ɕüəŋ ti] "younger brother" or [tà ì] "outline" with [tà i] "careless" (page 59) which are incontrovertible evidence that a large number (Cheng suggests perhaps several hundred) of Mandarin words are marked as having some kind of specification for neutral tone as they are abstractly stored in the speaker's lexicon or "dictionary". It is the examples of this second type, of course, which establish the questions and issues of considerable theoretical interest.

To affirm that the neutral tone in words like [ɕüəŋ ti] is not to be syntactically derivable, Cheng offers lists of examples (page 60) and elaborate arguments somewhat along the lines of the following. We might consider the words [p'ú t'au] "grape" and [puō li] "glass" as evidence

for a general rule that assigns neutral tone to second syllables of bi-syllabic words. But [má i] "ant" and [ní lún] "nylon" are at least two counterexamples. Or if the coordinate construction [iɛ̌n tʂiŋ] "eye" (literally "eye-pupil") suggests non-stress is assigned to second syllables of such constructions, [t'ú xuà] "picture" (i.e. "picture-picture") will offer an immediate disproof. And in this fashion numerous other potential neutral tone rules are eliminated from our serious consideration.

We have seen, then, that most neutral tone syllables have underlying basic tones which are converted through loss of stress in particular syntactic environments. And still other words can be assumed -- due to contrasting forms which are phonetically identical with an exception of the neutral tone syllable -- to be somehow marked for neutral tone in the lexicon. Cheng has been forced to consider the possibility, against which he musters up some tentative though inconclusive evidence, that particles and suffixes at least may not have any basic tone assigned in the speaker's dictionary of forms. But with this single possible exception of particles and suffixes, it seems altogether necessary to argue (cf. pages 66-67) that even in the lexical as opposed to the syntactic case of neutral tone items do not carry a neutral tone as such at the point of lexical representation but rather an exception feature which marks them as automatically subject to the Neutral Tone rule. That is, neutral tone syllables which are lexically determined come clearly from items specified in the lexicon with full tone plus the neutral tone exception marking -- a marking which specifies the item will always undergo the desired Neutral Tone rule. One piece of impressive evidence for a basic tone plus the exception feature is the alternate recorded pronunciations like [tʂié mù] and [tʂié mu] "program"

(and others given on page 66). We are in agreement here with Cheng (cf. page 67) that if a reading without a basic tone came from a lexical item without any underlying tone then there would be no reasonable way of claiming any relationship between obvious pairs like [tɕié mù] and [tɕié mu] which have identical meaning. It is more explanatory by far to conclude that such items have two forms within a lexicon of Mandarin (an individual speaker may have one, the other, or both), only one specified with some diacritic mark for susceptibility to the Neutral Tone rule. The fact that [ʂɨ̄ tau] "to know" exists alongside [pù ʂɨ̄ tàu] "do not know" is one of a number of further cases presented by Cheng (page 67) indicating that items with lexical neutral tone are carried in the lexicon with a full basic tone as well as a specification for neutral tone. And there are further problems with assuming that items such as those in Cheng's list in (119) (page 60) have neutral tone in underlying form. For one thing, any native speaker can employ contrastive stress for emphasis, as in the phrase [sɨ̀ iɛ̌n tɕīŋ búsɨ iɛ̌n tɕìŋ] "I meant the eye, not the eyeglasses". The native speaker must here know the original tone of the second syllable of [iɛ̌n tɕīn] "eye". This fact would be inexplicable if the second syllable were assumed to have an underlying neutral tone.

Now there does seem to be one worthy alternative solution to the one we are proposing here -- i.e. that lexical neutral tones must be specified by some kind of diacritic lexical marking of susceptibility to the Neutral Tone rule. One might want to suggest the possibility of some hierarchy of boundaries between the components of disyllabic words in Mandarin and further suggest that this hierarchy is somehow correlated with the separability of the syllabic components in terms

of semantics. The higher on the hierarchy is the boundary, the less applicable might be the Neutral Tone rule relative to the word carrying such a boundary (I am indebted to Joeann Paige for this suggestion). Thus the boundary marker between syllables in Cheng's list (119) might be for the moment considered as "weaker" (i.e. lower in the hierarchy) than such markers in list (118). Generally, a weaker boundary could be thought to indicate a closer semantic relationship between the components, which are thus less separable from each other. It is this possibility of a "semantic closeness" of syllables that Cheng's analysis does not take into consideration. But if this dimension of semantic closeness is at least temporarily adopted as plausible, then disyllabic words might be divided into the following three general types:

(a) Words with two full tones which are not syntactically derived, and whose components are not semantically close -- e.g. the words in Cheng's (118) and repeated below as (8). These words have an internal word boundary and are actually two words which always function as a unit in the native syntax. These are the "complex words" of a Structuralist's analysis.

(b) Words with one syllable in neutral tone which are not syntactically derived but whose components are semantically close -- e.g. the words repeated as (9). These words have seemingly a morpheme boundary only. Included here are reduplicated verbs with the sense of "try to" or "do a little", which are reduced by syntactic rule to single phonological words.

(c) Words of the type of [tɕiě tɕie] which are syntactically derived and (obviously) semantically close. These words have no internal boundary or perhaps only a formative boundary.

(8) FIGURE THIRTEEN

MANDARIN WORD LIST ONE

[má ɪ̌]	"ant"
[ní lúŋ]	"nylon"
[t'ú xuà]	"picture"
[tc'í kuài]	"strange"
[k'ǎu lǜ]	"to consider"
[tɕì ɕǜ]	"to continue"
[pàu kàu]	"report"
[tà ɕiàu]	"size"
[uèi ʂə̄ŋ]	"sanitation"
[láu pǎn]	"boss"

(9) FIGURE FOURTEEN

MANDARIN WORD LIST TWO

[p'ú t'au]	"grape"
[puō li]	"glass"
[iɛ̌n tɕiŋ]	"eye"
[xý p'iŋ]	"peaceful"
[pàu kau]	"the report"
[liɛ̀n ɕi]	"to exercise"
[mǎi mai]	"trade"
[ʂɨ̀ fei]	"scandal"
[fú ʂou]	"banister"
[láu xu]	"tiger"

The degree of closeness of constituency or the relative strength of boundary increases from word type (a) to word type (c). Words of types (a) and (b) are phonologically the same with regard to the Tone Sandhi rule; words of types (b) and (c) are identical with regard to the neutral tone. The Neutral Tone rule could be assumed under this tentative proposal to apply to compounds but not to complex words. It would be the salient feature of such a proposal that it is the boundary between syllables which is critical to either the application or non-application of the rules.

This type of argument is the only alternative of which we are at present aware to the kind of lexical exception marking we have been proposing. Yet at the same time it is aesthetically unpleasant and quite easily falsifiable. Impressionistically, any appeal to a dichotomy of word boundary versus morpheme boundary in Mandarin is sufficiently dangerous. But more importantly, such an analysis would be difficult to apprehend in light of forms like those given by Cheng as (118G) and (119E) -- [pàu kàu] (noun with basic tone) and [pàu kau] (verb with neutral tone). Can such words mean, then, that verbs have a weaker boundary than nouns? Then why is it that other verb forms appear in the list (118) while nouns are also found throughout (119) (see page 60)? And such a solution, it can also be noted, would give no explanation at all for the pairs found in Cheng's example (137) (page 66) like [tcié mù] and [tcié mu] for "program" and [ɕiōu lǐ] and [ɕiōu li] for "to repair" where boundaries appear totally irrelevant. The lexical exception filter would, however, as already suggested, quite adequately explain all such forms.

We have been led to an observation, then, that Cheng's proposals

regarding the Mandarin neutral tone not only account in a fully explanatory way for an important phonological phenomenon of Mandarin but also offer an independent support for an important recent modification in the standard version of generative theory. In taking up the question of the type of morphology proposed by Morris Halle, Botha (1974) has argued on the basis of the evidence from Afrikaans nominal compounds that Halle's lexical exception filter must in actuality be a surface structure exception filter. But the evidence surrounding the Mandarin neutral tone makes this highly implausible as the Mandarin speaker's ability to distinguish between otherwise identical words on the basis of a neutral tone which is clearly not derivable syntactically must be based on access to information that precedes the point of lexical insertion in his grammar.

NOTES

[1]As far as I am aware, Stampe's single reference to rules of a "morphological" type is a brief footnote comment found in Stampe 1972.

[2]Henderson introduces the argument that "lexical redundancy" and "low-level segmental redundancy" are confused within an SPE model and then sets out to clarify distinctions between the two:

> The rules which fill in the blanks by adding values to matrices will be called LEXICAL REDUNDANCY RULES, or L-RULES; and those which add whole features such as [tense] will be called SEGMENTAL REDUNDANCY RULES, or S-RULES. Thus it is an L-Rule which adds the value minus to the feature [high] for /a/; it is an S-Rule which states that the systematic phonetic representatives of /e/ are [+tense] in stressed syllables and [-tense] in unstressed syllables. S-Rules have traditionally been thought of in generative phonology as "detail rules" (Postal 1968); but as seen in [above], they are actually phonetic redundancy rules, in that they add features which are not part of the definition of phonemes (1976, page 316).

Henderson's criticism of SPE and proposal for solution are as follows: "In assigning values to features with no phonetic correlates in certain major classes, absolute marking conventions create unnatural classes and unpronounceable segments; their output should be expresses in u's, and interpretive marking conventions should assign values only to meaningful features" (1976, page 314). Given the questionable status of the marking conventions and markedness theory per se, Henderson's article retains primarily an historical interest, as a competent and thorough review of several earlier approaches to markedness notation.

[3]To emphasize this interpretation of Stampe, we may quote the following passage from Patricia Donegan Miller's assessment of Natural Phonology:

> Both the implicational hierarchies and the markedness conventions, however, are metalinguistic frameworks -- they impose abstract constraints on phonological systems from outside. A desire to derive the constraints from within the phonological systems themselves has led David Stampe to propose, instead, "an innate system of phonological processes which resemble the implicational laws and markedness conventions in content but have the same ontological status as the natural processes (so-called "rules") of the phonological system of any individual language (1972a, page 136).

[4]This article by Clayton is a reworking (with little modification) of an earlier chapter of her dissertation. She acknowledges Kaye's

examples of apparent MSCs, such as that from Odawa mentioned at the end of this current chapter, but offers little in the way of serious rebuttal.

[5]It is still largely true as Hooper (1972, page 525) has contended that "although the syllable is generally recognized as an important phonological unit and is often mentioned in phonological studies, current phonological theory provides no definition of the syllable, and thus no formal means of referring to it as a unit."

[6]Halle's rationale for lexical redundancy rules in found in Halle 1959 (pages 29-30) and is also quoted at some length by Clayton (1976, page 296).

[7]Halle's distribution of rules specifies that MS rules are those to apply before transformations and P rules are those which apply after the transformations have taken place.

[8]This is, recognizably, a summary of the interpretation given also in Stanley 1967 (page 396).

[9]The issue of ordering paradoxes which arise inevitably in the standard treatment of redundancy rules is perhaps most thoroughly discussed in Chapter Three of Clayton's (1974) dissertation.

[10]Some of these examples are from what Harris labels andante and others from so-called allegretto (fast) speech styles.

[11]The use of this line of argument here is not original. E.g. see also the discussion in Hooper 1975 (passim).

[12]As Wojcik puts it,

> In fact, SGP [Standard Generative Phonology -- PCB] must devise a rather complicated theory of markedness to ensure that grammars containing Nasal Assimilation or Final Obstruent Devoicing are considered more natural than those that lack them. This is why Chomsky and Halle (1968: 400) begin chapter nine of The Sound Pattern of English with an apology for a "fundamental theoretical inadequacy" in their work and proceed to respond to that inadequacy with markedness conventions and linking rules. Stampe responds with a fundamental change in the theory, and the need for markedness disappears (1975, page 11).

Part Three

The Applications of Natural Phonology

CHAPTER FIVE

NATURAL PHONOLOGY IN THE MIAMI CUBAN DIALECT

5.1 Introduction

A partial purpose of this dissertation is to record the effects of native phonological processes upon the nativizations of representative loanwords in Miami Cuban Spanish (MCS). It is hypothesized in these chapters that nativizations of this type will argue strongly for the theory of phonological processes espoused in the Natural Phonology developed by David Stampe.

In carrying out such a program it is a primary prerequisite, then, to establish an overview of the native innate system of processes which will characterize the Miami Cuban dialect of Spanish.[1] The purpose of this fifth chapter will be threefold: (1) to expand on several controversial issues surrounding description of the native phonological component of Cuban Spanish; (2) to provide a list of natural allophonic processes (often mistaken in earlier studies for the conventional type phonological "rules"), such a list being intended primarily as a point of reference for the discussion of loanword phenomena in Chapter Seven; and (3) to consider at greater length two proposals from Hammond 1975 and Hammond 1976 (viz. (a) phonemic restructuring in MCS of the system of voiced obstruents and related loss of the process of Spirantization, and (b) emergence of a velar nasal phoneme for this dialect and consequent loss of the process of Nasal Velarization) which bear directly

on the analyses of loanword data presented in Chapter Seven and which stand as theoretically inadequate proposals once the analysis by Hammond is replaced with the possible Natural Phonology interpretation of MCS.

Although phonological rules and processes familiar in the standard dialects of Spanish (e.g. the Spanish of Mexico City which is described by Harris 1969, in what remains the most influential generative phonological study) are generally shared by MCS -- the phonological processes of Velar Softening,[2] Glide Formation, Syncope, Obstruent Voicing, Nasal and Lateral Assimilations, and Degemination, among others -- it is a wider application than in the standard Madrid dialect or highlands Mexican dialects of approximately a dozen additional processes which gives to the Miami Cuban dialect its distinctive and recognizable character. Among the processes requiring some special discussion in this present chapter are those of Velar Nasal Intrusion (Hammond 1976, Section 3.3); Intervocalic Obstruent Voicing (Saciuk 1974); S-Aspiration and S-Deletion (Terrell 1974a, 1974b, 1975b and Hammond 1976, Section 3.5); Vocalic Reduction and Final Devoicing (Hammond 1976, Section 3.8 and Cárdenas 1970); Final Consonant Deletion (Hammond 1976, Section 3.6 and López Morales 1965, 1970); and Spirantization (Hammond 1975; 1976, Section 4.9.3).

5.2 Issues in Cuban Phonological Studies

A common assumption in the literature about Cuban Spanish and about the regional Spanish of the Antillas in general is that these dialects are characteristically "easy-articulation" dialects, displaying a lethargic and universally-relaxed mode of speech. Although there is factual basis for this dialectal classification, the descriptions and

accounts of such phenomena have often been couched more in the terms of folklore than phonology. For example:

> The generalizations about Antillean Spanish have in the past alleged a universally relaxed, much less tense articulation of non-syllabics, attributing coincidence of such to the so-called tropical lethargy induced by warmth, unvarying good climate, and a degree of social informality (Lamb 1968, page 125).

It is precisely this observed tendency toward a reduced articulatory effort, however, which makes native phonology as well as loanword phonology in these dialects of special interest to any discussion of Natural Phonology.

Universal acceptance of assumptions about the general character of Antillean Spanish has not resulted, however, in very much consensus of opinion among linguists working with Cuban Spanish on particular details of even the most generally-observed phenomena. Though most agree that Cuban Spanish in particular is distinguished by its preference for such weakening phenomena as aspiration or deletion of syllable-final phonemic /s/ and the intervocalic voicing of obstruents, these processes (or rather what has usually been discussed is the surface manifestations -- which allophone occurs in what environments -- and not the processes themselves) have as the norm been given inconsistent and contradictory descriptions. Most especially surrounded by contradiction and confusion are discussions in the more recent literature (esp. Terrell 1974a, 1974b, 1975c; Guitart 1973; and Hammond 1976) on aspiration and deletion of /s/, on the widespread velarization of nasals which reaches unique dimensions in Cuban Spanish, and on assimilations of liquids /l/ and /r/ to following consonants (described as a phenomenon of gemination in Guitart 1973).[3] To establish this point, it is worthwhile surveying a number of the more pronounced discrepancies in these analyses.

With regard to the weakening involving sibilants and nasals, we are most fortunate to possess for comparison several studies by Terrell and Hammon with superficially (though not actually) parallel methodologies and yet disquieting variations in the results. Though there are identifiable differences here in techniques of data gathering and in the nature of the sample tested which most evidently, in part at least, account for the differing results, it is precisely this lack of parallel studies and results which is at issues. Hammond makes detailed comments on these contrasting studies in Sections 2.2.1 and 2.2.4 of his dissertation (Hammond 1976) and provides the only readily available source of this data (since Terrell's work remains in large part unpublished). For our purposes here an admittedly superficial treatment of Hammond's presentation should be adequate to establish areas of disagreement and discrepancy.

Both Hammond (based on five hours of recorded interviews with twenty-one Miami Cuban informants) and Terrell (based on twenty hours of recorded interviews with newly-arrived Cuban exiles) tabulate the percentages of occurrence of [s] and its alternative realizations as [h] and [Ø] in the following utterance positions: syllable-final position within a word, word-final position (within a breath-group for Hammond), and absolute final position (for Hammond only). The somewhat surprising results are presented as Figure One below (drawing the data as given here from the account in Hammond 1976, pages 75-79).

(1) FIGURE FIFTEEN

CUBAN SPANISH ASPIRATION DATA

Havana Cuban
TERRELL 1974a
Percentages of Occurrence of s-retention and s-aspiration or deletion

	[s/z]	[h/ø]
Syllable-Final (Within Words)	3%	97%
Word-Final	33%	67%
Word-Final (Before C)	5%	95%
Word-Final (Before V)	30%	70%
Pre-Pausal	62%	38%

Miami Cuban
HAMMOND 1976
Percentages of Occurrence of [s] and [h] and [ø]

	[s]	[h]	[ø]
Syllable-Final (Within Words)	9.4%	70.3%	20.3%
Word-Final (Breath-Group)	2.8%	43.0%	54.2%
Word-Final (for other word-final occurrences within a breath-group Hammond gives percentages of occurrence before specific consonantal segments)			
Absolute Final	3.8%	21.8%	74.4%

In Terrell 1974b, on the other hand, additional composite percentages are given, based on a total of 7149 occurrences of /s/: [s] = 17%, [h/ɦ] = 63%, and [Ø] = 20%. We should compare with these figures the overall percentages recorded by Hammond, based on about half as many (3415) total occurrences: [s] = 4.7%, [h] = 46.2%, and [Ø] = 49.1%.

Such diverse findings work to recall and reinforce the reliance on guesswork and speculation which is an all-too-regular feature of the earlier studies in Cuban linguistics. Of course apparent explanations present themselves for a number of the discrepancies in the comparisons summarized as Figure One. As Hammond (1976, page 76) takes into view, comparison at best is perhaps invalid (if not meaningless) since the two studies explore differing linguistic environments -- for Terrell: (1) syllable-final in word-internal position and (2) word-final (before V, C, and pause); for Hammond: (1) syllable-final in word-internal position, (2) word-finally within a breath-group, and (3) absolute final position -- and only in the first instance do these environments precisely coincide. Also, Terrell considers only two divisions of the data: s-retention ([s] or [z]) and s-deletion ([h], [ɦ], or [Ø]); yet Hammond considers three: retention, aspiration, and deletion. Terrell, to carry the contrast yet further, is guided by a different apparent interpretation of s-deletion, interpreting replacements of V[s] sequences with long vowels to be aspiration and not deletion (Hammond 1976, page 77). Perhaps most important though, Terrell's data results from more "formal" interview environments, involving newly-arrived Cuban exiles (see note sixteen from Chapter One) recorded within hours of their disembarking in an unfamiliar and potentially hostile setting, while Hammond's field work consisted of "informal" taping sessions

designed to gather relaxed and uninhibited fast speech data. This undoubtedly had something to do with the overall percentages (17% for Terrell and 4% for Hammond) reported in regards to s-retention.

A number of further observations seem mandatory. One is the questionable validity of Terrell's percentages, considering together his interpretations of aspiration and his sources of data (fast speech processes are unlikely in formal interviews). Another is the highly tentative nature as well of Hammond's findings in light of the limited scope of his study and our imprecise knowledge of the many variables involved in such statistical compilations. Would another randomly selected twenty speakers provide anything like these percentages? A firm conclusion would have to be, however, that Cuban Spanish does have a clearly verifiable and active process of s-weakening, in which aspiration is presumably one related step: underlying /s/ does not normally occur (at least for Miami and Havana speech) in any non-syllable-initial environment at the surface level (Hammond 1976, page 139) and [h] and [Ø] occur with approximately equal overall frequency in about 95% (Hammond's percentage) of the recorded cases. One final conclusion from the data compiled by Hammond and Terrell, then, is that those who persist in contending that syllable-final s-deletion is "vulgar", "low", "uneducated", or "Black Cuban dialect" (viz. see Guitart 1974) are simply unaware of linguistic facts.

A similar comparison with similar attending discrepancies and connected difficulties can also be made of Terrell's (1975c) versus Hammond's (1976) frequency studies on occurrence of the surface nasal forms. Terrell has reported that in syllable-final position word-internally his survey (same informants) reveals 84% standard nasal

assimilation, while only 16% nasal deletion with concomitant nasalization of the preceding vowel (which leaves less than one percent for the surface manifestations of a repeatedly-noted Cuban syllable-final velar nasal). For word-final environment within a breath-group Terrell reports the following percentages: 3% alveolar (unmarked) nasal, 33% nasal assimilation, 38% nasal deletion plus vowel nasalization, and 26% velar nasal. The percentages recorded by Hammond stand once again in sharp contrast to those of Terrell. In word-internal syllable-final environments: 31.0% nasal assimilation, 26.8% deletion and accompanying vowel nasalization, and 42.2% velar nasal. In word-final position within a breath-group: 3.4% nasal assimilation, 33.7% deletion and vowel nasalization, and 62.9% velar nasal.

Hammond (1976, page 42) speculates on a number of variables, which again might be responsible for yielding such dramatically different statistical results, and arrives at the most reasonable hypothesis as being that it is simply difficult for the linguist to distinguish and record with any certainty the resultant nasalized vowels as opposed to full-fledged velar nasal segments.

To accentuate now a more positive feature of these recent works, an altogether unprecedented feature of both Terrell's and Hammond's studies in light of previous work on Cuban phonology has to be their laborious attention to thoroughness and detail. The notable shortcoming of all previous work making any pretense to phonological description (since most earlier studies are no more than lexical compendia) has been the willingness to draw sweeping generalizations on the basis of a handful of informants or only a casual acquaintance with the data. There are a number of outstanding examples.

Iṣbasescu 1968 (discussion in Lamb 1968, pages 20-24, and Hammond 1976, pages 32-39) offers conclusions about the general pronunciation of insular Cuba based on a study of only six informants, who happened to be students in Bucarest in 1964 and who represent only for of the six Cuban provinces. No information about the number ot type of informants (see note fifteen of Chapter One) is provided in the dissertations by either Sosa (1974) or Guitart (1973). The latter, at least, apparently has generalized from intuitions about his own personal idiolect to create an amorphous and perhaps fictional dialect which he designates as the Educated Spanish of Havana (ESH). Guitart's work contains no mention of methods of data-gathering and appears largely impressionistic in bent. And although Lamb (1968) bases his dissertation on transcriptions of interviews with thirty native informants, all had been resident in the Chicago area for a number of years and therefore provide a questionable sample on which decisions might be drawn about a current Havana dialect of Cuban Spanish.

This last observation has long been an unavoidable political and linguistic fact of Cuban phonological studies. All three major dissertations on Havana Spanish (Lamb 1968, Guitart 1973, and Sosa 1974) have been forced to rely for informants on exiled Cuban residing in the United States, and only the studies of Terrell, Haden and Matluck (1973), Vallejos-Claros (1970), and others who have utilized the tapes of the "Coordinated Study of the Linguistic Norms of the Principal Cities of Ibero-America and the Iberian Peninsula" might be safely said to base their work on uncontaminated Havana speakers free form English interference or other dialectal influence.

It might be asked, then, if any comparison like that implied above

between the studies of Terrell (Havana informants) and those of Hammond (Miami informants) is well motivated: are they not examining different Cuban dialects, which fact itself accounts for the diversity of the results? Or putting it in slightly different terms, it might be proposed that any criticisms of Guitart's (or Terrell's) conclusions about consonantal processes in Havana Spanish based on my own field work, say, would bear little weight, since Miami Cuban Spanish after all is not Havana Cuban Spanish.

I would wish to offer at this point the following observation: that in terms of the major phonological features we are discussing with this and the following two chapters, the two dialects are indeed substantially similar enough to allow for intimate comparisons. I base this assumption on at least the following set of general observations:

(2) Those phonological phenomena being discussed here -- s-aspiration, velarization, intervocalic spirantization and deletion of obstruents, etc. -- are those for which there is not even a slightest hint of any possible influence from English. The phonological processes in question are in each case long-attested and internally well-motivated processes of Spanish itself.

(3) The informants in all these studies were born in Cuba and despite foreign residence continue to employ Spanish as their primary language and to adopt the Spanish-speaking culture as their primary culture. Miami Cubans continue to be immersed in a heavily "Cuban" and not just "Latin" subculture, and one in which Cuban-style Spanish is still the norm.

(4) As Guitart for one has also informally noted, the contact situation in Miami (and probably elsewhere throughout Cuban colonies

in the United States) has led to considerable lexical borrowing but little direct influence on the native Cuban phonological system: i.e. Miami Cubans in their Spanish (even if they are largely bilingual) pronounce the English borrowings as though they were actual Spanish words and only rarely (among the group who are truly bilingual) adopt any measure of the source language phonology (see discussion of this phenomenon with Chapter Seven).

As I have observed similar phenomena in MCS which parallel those reported and discussed by Guitart, Terrell, Sosa, Lamb, etc., and as the differences in data (where they do exist) are not attributable in any apparent way to the influences of English, I will assume for the remainder of this discussion that observations from Guitart and Terrell, and others drawn generally from Havana Spanish, are in large part applicable to Miami Cuban speakers as well. Also, theories projected by those studies should be expected to stand up under the weight of evidence culled from this dialect as well as they stand up to the data reputedly (though apparently not always -- i.e. Guitart) based on the insular dialects alone.

The claim here is certainly not that MCS is Havana Spanish in all of its features (compare Hammond 1976, page 7). But rather, the major phonological processes being discussed here are, until shown otherwise, to be taken as being to some greater or lesser degree characteristic of most dialects of Cuban Spanish (viz. s-aspiration, deletion of final consonants, velarization, etc.) and not the exclusive feature of any single isolated dialect.

Having put to rest a necessary digression, let us now return directly to an even more useful case of inconsistent and unsupportable

conclusions about Cuban phonology. These are the observations drawn from Guitart's (1973) doctoral dissertation on a proposed Educated Spanish of Havana (ESH) -- a mode of speech which I take to be largely a fiction as a legitimate unified dialect, though this is not the issue of immediate concern to us here. The issues taken to be most controversial are those involving (1) neutralization of nasals, (2) neutralization of strident obstruents, and (3) neutralization of liquids. Guitart's claims about the extremity of such neutralization processes is simply not borne out by the data at hand, at least not with the educated speakers of Miami Spanish observed by this author, the wide majority of whom are natives of Havana (and thus presumably originally speakers of ESH).

The data offered in Terrell 1975c as well as in Hammond 1976 is convincing that velar nasals in syllable-final and word-final positions in Cuban Spanish are at least predominant if not the general rule. It is a most familiar generalization about Spanish phonology, by contrast, that all Spanish dialects display a general rule of Nasal Assimilation to following consonants as illustrated below in (5). However for Cuban dialects the forms in (6) are also commonplace (cf. Hammond 1976, page 210, where the following forms are listed):

(5) GENERAL SPANISH NASAL ASSIMILATION

un bote	[umbóte]	un gato	[uŋgáto]
un fósforo	[umfósforo]		
un diente	[undyénte]		
un cigarro	[unsiɣáro]		
un chico	[unčíko]		
un llavero	[uñyaβéro]		

(6) CUBAN SPANISH NASAL VELARIZATION

un boleto	[uŋboléto]	[uᵐŋboléto]
un francés	[uŋfransés]	[uᵐ̆ŋfransés]
un domingo	[uŋdomíŋgo]	
un señor	[uŋseñór]	
un chiste	[unč̆íste]	
un caballo	[unkaβáyo]	

(the transcriptions at the far right being those given in Guitart 1973, page 48)

Early studies of Cuban phonetics (see Lamb 1968, Chapter Two for more detail) have sporadically recorded the impression that in these dialects as in other American dialects the velar nasal phone may occur in environments not accounted for by (actually in violation of) this standard Spanish assimilation rule (as illustrated by (6) above). In certainly the most extensive study of this phenomenon to date, Hammond (1976, pages 105-27, 228-33) has now provided some indication of the circumstances under which this velarization applies and the constraints (operative at least for MCS) by which it is governed. Among Hammond's informants, a rule of Nasal Velarization was in evidence for 83.0% of all nasals in absolute final position, for 62.4% of all nasals in word-final position, and in 42.4% of all the syllable-final cases. These findings lead Hammond to speculate that the Nasal Velarization rule has some clear correlation to the strength of boundary involved (page 117). Two constraints on this phenomenon seem especially noteworthy. Cases of the velar nasal in syllable-initial position occurred only most rarely (a dozen or so occurrences in total). And before following dental segments, [ŋ] was recorded

98.9% of the time before [t], yet in only 1.1% of the appearances before the voiced segment [d]. Some still unexplained condition on the rule seems to be in effect here. It can also be further pointed out that an apparently related rule of nasal consonant deletion (preceded by obligatory vowel nasalization) was very much in evidence for Hammond's informants, being observed 9.7% of the time in absolute final position, 33.7% in word-final position, and 26.8% in syllable-final position.

Guitart's description of nasal phenomena in ESH is diametrically at odds with Hammond's statistically-more-elaborate account of surface nasal phenomena in MCS. Guitart views a set of characteristics which surround nasal consonants to be part and parcel of the general set of characteristics establishing the behavior of consonants and liquids language-wide when they fall in syllable-final position before other consonants:

> In ESH, nasals contrast in initial position and intervocalically, as in other dialects. In utterance-final position, however, it is only the velar nasal that occurs, without exception. It can be said that one of the characteristics of ESH is a strong tendency toward velarization. This tendency is manifested in closed syllable-final position, where every nasal, regardless of the consonant that follows, is normally velarized, although assimilation also takes place in some cases, resulting in a sort of co-articulation where a dorso-velar element is always present [see examples in (6) above -- PCB]. A distributional statement of ESH nasals in closed syllable-final position may be formulated as follows: the velar nasal occurs before dental and alveolar and alveolo-palatal consonants, and, of course, before velars, while a velarized labial nasal occurs before labial and labiodental consonants. Thus, given the following utterances: (A) un boleto, (B) un francés, (C) un domingo, (D) un señor, (E) un chiste, (F) un caballo, the n will be realized as [ŋ] in (C), (D), (E), and (F); as $\underset{[\eta]}{m}$ in (A); as $\underset{[\eta]}{\tilde{m}}$ in (B) (1976, pages 22-23, with my emphasis -- PCB). [Note that the forms cited as examples in this passage are those already given (as quoted by Hammond) in (6). For discussion of these same forms also see note four at the end of this chapter.]

Rejecting out of hand any assumption that ESH should be expected

to be distinct from MCS in some way just such as this (having already established a rationale for making such a rejection), we might suspect immediately that Guitart's claims are to be judged extreme and maybe even implausible in the light of the data-oriented treatments like those given by Hammond and Terrell.

Guitart offers no hard-core data, and there is nowhere in his study any appeal to anything like spectrographic evidence in support of these or any of the even more complex series of co-articluations reported (such as those given e.g. on page 24 resulting from the neutralizations of liquids and featuring a "dorsopalatoalveolar" stop before dentals and alveolars other than s). It is speculative at best to claim such segments on the basis of the purely impressionistic evidence recorded by a single observer.

Regarding my own field work, twelve MCS informants (seven from the province of Havana as described in Appendix Part One) were asked to pronounce rapidly, three times in succession, Guitart's six "article+noun" sequences found in the passage just quoted (and repeated in (6) above), and the results generally confirmed the rule of assimilation in as many cases as the rule of velarization (a tentative result, admittedly, in light of the nature of the test instrument, yet not an entirely insignificant one).[4] Throughout the remainder of my tapes, examples of velarization have been frequent but not exceptionless. The rule of velarization applies and fails in all speech styles and at all rates of speed, and often the Cuban speaker (MCS at any rate) velarizes and assimilates different nasals within the same word or phrase. Both a rule of nasal assimilation and a rule of nasal velarization, then, appear to be extremely optional ones for Cuban speakers (just

how optional is something we may be a long way from determining). This last observation should pack considerable weight in light of our discussion of the optional nature of innate processes in Section 2.2.2 and we will be returning to it presently when we set out to defend a catalogue of MCS natural processes below. If Nasal Velarization is other than an optional process in any of the several American Spanish dialects where it occurs (see note three), it will require much less "impressionistic" dialect studies than Guitart's to convince us that this is so.

Guitart's view of nasal phenomena has an added significance since he relates it to a claim that all strident obstruents as well as all non-strident obstruents similarly become velars (i.e. [+back]) in closed syllable-final position. And this in turn is cited as support for a general theory of Relative Markedness within the framework of generative grammar: viz. in positions which are phonetically-neutralized environments the physiologically "unmarked" elements are the norm, and therefore in closed syllable-final position the preference for [+back] segments (which presumably are somehow easier to articulate) now achieves something of an explanatory value. For the purposes of the present chapter, however, we will concentrate only on the purely language-specific implications of Guitart's proposal.[5]

Guitart has tried to relate physiological "markedness" to other types of "markedness" in order to account for the preference for velar nasals in closed syllable-final position in ESH. Ease of articulation seems to be the dominant issue, since

> it seems correct to assume that the set of instructions involved in realizing both /m/ and /n/ as [ŋ] is simpler than the one needed to realize /m/ sometimes as dental, other times as alveolar, other times as interdental etc., and also simpler than

> the set specifying /n/ sometimes as labial, other times as palatoalveolar, other times as palatal etc. (1976, page 76).

In the related case where a co-articulation of labial and velar purportedly takes place before a labial consonant, "given that the nasal is going to be realized as [+back] anyway, one should think that it is simpler to realize a two-segment cluster in which the members agree with respect to x features than it is to realize a sequence in which the members agree with respect to x+1 features" (1976, page 77).

Since there are dialects in which a still more radical and more satisfactory solution to articulatory ease is total deletion of the nasal segment (cf. Hammond's observations on Guitart's analysis below), Guitart is forced to concede that there can be no proposition that velar nasals in syllable-final position constitute the minimum in neurophysical effort (1976, page 77). Total deletion is not encompassed within Guitart's approach, which is concerned rather narrowly with presenting a notion of physiologically "unmarked" as a necessary alternative to an earlier version of generative grammar which took psychologically or perceptually "unmarked" as the single criterion governing segment inventories.

The motivation for velarization, when any persistent target of articulatory ease would more likely favor deletion, looms as a considerable problem for the system of generative phonology advocated by Hammond as well, and one we can return to again more profittably in light of the alternative analysis of Natural Phonology brought up below.

It is this identical principle of ease of articulation (meaning

apparently for Guitart some kind of economy in neutral instructions) which we are to assume lies behind the rule in ESH which in rapid speech converts non-strident obstruents as well to [+back]. E.g. absoluto [aksolúto], étnico [égniko], and admitir [agmitir]. The companion rule also rendering strident obstruents as [+back] is given a somewhat more explanatory treatment.

> Perhaps it could be argued that [h] is less complex, i.e. more natural, than either [s] or [f] from a physiological point of view. In the first place, one would think that the greater noise intensity of both [s] and [f] vis-à-vis [h] is the product of a relatively greater degree of articulatory effort. Secondly, it seems that the production of either [s] or [f] is mechanically a more complex process than that of [h], for in the stridents there is an additional obstacle involved: the lower teeth in [s] (in addition to the constriction made by the tongue) and the upper teeth in [f] (in addition to the lower lip) (1976, page 74).

Taking all the nasal and obstruent neutralization rules together, Guitart claims to achieve an important generalization about the phonology of ESH: all non-liquid consonants are realized as either velars or pharyngeals in either syllable-final or word-final position (1976, page 73). The choice between velar and pharyngeal is a function of whether or not the underlying segment is strident (strident consonants becoming pharyngeal and non-strident ones velar).

Again with the obstruents, identical to the nasals, Guitart must admit that in such neutralizations the maximal reduction of neurophysical effort is not what is being achieved here. In addition to aspiration as a target output in the treatment of final /s/, deletion is rampant for this and other Cuban dialects (Hammond reports 75% deletion for absolute-final position among his MCS informants).[6] And for the non-strident obstruents Guitart notes that "in certain Cuban dialects, for instance, the realization of a non-strident ob-

struent in syllable-final position is zero in rapid speech; e.g. [dotór] for 'doctor'" (1976, page 77).

A further issue resulting from claims introduced in Guitart 1973 involves the similar neutralizations reported for liquids /l/ and /r/ in syllable-final position. Guitart contends that while for numerous Spanish dialects (Andalusian, Puerto Rican, and generally throughout Spanish America) the distinction between liquids is regularly lost in syllable-final position, for ESH this type of neutralization is limited to closed syllable-final position before only certain consonants. The distribution, as Guitart interprets it, can be described as follows:

> On the other hand /l/ and /r/ are both realized as an unreleased, voiced dorsopalatoalveolar stop (represented as $\ddot{d}^{7}$) before dentals and alveolars other than [s], affecting these in the following manner: (a) sounds that would normally be apicodental in that position, i.e. [t,d] are realized instead as dorsopalatoalveolar, with the voiced apicodental -- normally, a fricative before [r] -- being released always as a stop; e.g. [ad^{7}de, ad^{7}te, sad^{7}do, ad^{7}to] for arde "it burns", arte "art", saldo "balance", alto "tall"; (b) the apicoalveolar nasal is realized instead as a dorsopalatoalveolar nasal; e.g. [kad^{7}ñe] for carne "meat". Before labial, velar, and pharyngeal obstruents and before the other nasals (i.e. [m] and [ñ]) l/r becomes like the consonant that follows in manner and point of articulation with true gemination occurring before [m], [ñ], [f], and [x]; in all other cases the realization is an unreleased voiced stop, and if what follows is a voiced oral consonant, the latter is realized as a stop rather than as a continuent (1976, page 24).

Here Guitart gives examples like [eb^{7}bé^{7}de] for el verde, [eg^{7}káso] for el caso, and [seb^{7}pádre] for ser padre. Three features must be noted about such examples. First and foremost, there is no apparent extant spectrographic evidence for what are reported as being highly-complex co-articulations. Nor do we have any indications given of what percentage of the total articulations of such combinations by speakers of ESH these transcriptions would represent. Is it claimed that ESH speakers make these and only these co-articulations in every

utterance of such clusters? This appears to be Guitart's observation. Or do they make them 20% or 60% of the time? Or only sporadically? The possibility that these are the only assimilations involving these segments seems remote. And finally, in light of the difficulty of the task facing the linguist who attempts to distinguish between, say, the velar nasal phone and a nasalized vowel (with the following nasal consonant deleted), it would seem that the claim that an unreleased voiced dorsopalatoalveolar stop is clearly distinguishable in all cases of l and r before dentals and alveolars other than with /s/ (with this second segment also being released as dorsopalatoalveolar) stretches the linguist's credibility.

So far we have been questioning the validity of Guitart's data, or at least a degree to which it is representative of generalized phenomena in Cuban phonology. But still there is one more central theoretical issue raised by Guitart's treatment of neutralization phenomena which demands some comment in this chapter. This involves the nature of the generalizations (in terms of phonological rules) which he formulates in order to capture the nature of nasal, obstruent, and liquid neutralizations as the evident features of ESH phonology.

The first thing that is notable in Guitart's framework is the proliferation of assimilation rules of structurally similar types. At first blush there are functionally similar and yet structurally distinct rules for behavior of non-strident obstruents before any consonant not in the same syllable, strident obstruents syllable-finally, and nasals in closed syllable-final position. Since an apparent generalization seems to be missing -- a distastful circumstance in generative grammar -- the treatment of these neutralizations as separate phenomena would

seem highly undesirable. Using generative notation for collapsing of rules, then, along with certain generalizations about surface constraints in Cuban Spanish (e.g. word-final [h] and [ŋ] before a vowel are resyllabified and become syllable-initial, no non-strident obstruents occur in word-final position etc.), Guitart achieves the following phonological rule (7) which accounts for all cases of velarization or pharyngealization of non-liquid consonants in syllable-final or word-final position. The strident consonants (s and f) become pharyngeal (h) and non-strident consonants (nasals and stops) become velars.

(7) ESH CONSONANT NEUTRALIZATION RULE

$$\begin{bmatrix} -\text{Syllabic} \\ +\text{Nasal} \\ -\text{Sonorant} \\ @\text{Strident} \end{bmatrix} \rightarrow \begin{bmatrix} +\text{Back} \\ -\text{Mid} \\ -@\text{High} \end{bmatrix} / \underline{\quad} \begin{Bmatrix} \$ \\ \# \end{Bmatrix}$$

The formulation of a single neutralization rule (for all its obvious simplicity and therefore attractiveness within the generative framework) is objectionable on two counts. First, there is the notion that these natural and physiologically-motivated processes of weakening through velarization and pharyngealization (see discussion below) seem to be somehow oddly characterized by such highly cognitive grammatical properties as the bracket notations, Greek letter variables, and other abbreviation devices characteristic of generative rules like (7) above. When faced with a similar analysis, Stampe (1973a, pages 47-49) suggested a reinterpretation of the English historical trisyllabic shortening rule in order to capture a much simpler process actually operative, which is sensitive to syllable boundaries, phonetically motivated, and indicative of the infant origins of the substitutions involved. Guitart's rules for non-liquid (7) and liquid (1976, page 79) neutral-

izations since they are achieved by means of collapsing devices and motivated by feature-counting simplicity criterion are open to the same objections.

Furthermore, Guitart's neutralization rule (7) is also a product of the assumption that a single process (i.e. a significant generalization of ESH) of consonant neutralization is actually involved. The claim implicit here, then, is that aspiration in the case of syllable-final or word-final /s/ and /f/, realization of all remaining obstruents /ptkbdg/ as velars in the same environments, and the velarization of nasals are all manifestations of a single generalized process. An unquestioned acceptance of Guitart's claim that such neutralizations are achieved unfailingly in all occurrences might lend credence to such a claim. Yet if Hammond's elaborate study of percentages of occurrence (or my own statistically less thorough observations of speakers of MCS) can be taken as indicative of the general trend in Cuban dialects, the the claims for a single process would seem considerable less defensible.

Among Hammond's informants e.g. nasal velarization occurs in 83.0% of the total cases in absolute final position and only 41.8% of the cases in syllable-final position (where it is in conflict with the standard nasal assimilation process). s-aspiration (as opposed to the more frequent phenomenon of deletion), however, occurs only 21.8% of the time utterance-finally; also, among Guitart's informants cases of f-aspiration rarely occur in comparison with s-aspiration. Such data, then, is hardly consistent with the concept of a single identical process occurring in all instances; nor is it possible to see how these weakening phenomena (however much more regular they are in ESH) should be taken as a single process involving all consonants in one dialect

but a collection of similar yet distinct processes in all related dialects. Also, the process of velarization in nasals seems much more closely akin to the connected processes of Vowel Nasalization and subsequent Nasal Deletion than it does to, say, pharyngealization of syllable-final /f/ -- a fact which is not captured at all by Guitart's single generative type rule.

These and other similar difficulties (such as Hammond's concern over the motivation for nasal velarization or the explanation for rampant spirantization of obstruents) all seem to disappear readily enough, however, once we take the various weakening phenomena of Cuban dialects to be a series of teleologically related innate natural processes active in the grammars of adult speakers yet having their origins in infant speech strategies. The discussion in the final two sections of this chapter briefly characterizes a number of these processes and then takes up a few related theoretical issues.

5.3 Some Selected Processes in Miami Spanish

Consider now the following quite minimal list of active natural phonological processes in MCS, this partial listing containing processes especially relevant to the activities of loanword phonology (Chapter Seven) as well as to resolutions of several theoretical issues seemingly endemic to this selected dialect (subsequent section below).

(8) FIGURE SIXTEEN

SOME SELECTED NATURAL PROCESSES OF MCS

CONTEXT-FREE NATURAL PROCESSES

VOWEL DENASALIZATION	$\tilde{V} \rightarrow V$
GLOTTAL STOP DELETION	$ʔ \rightarrow \emptyset$
NASAL DEVELARIZATION	$ŋ \rightarrow n$

DESPIRANTIZATION

$$\begin{bmatrix} C \\ +\text{obstruent} \\ +\text{continuant} \end{bmatrix} \rightarrow \begin{bmatrix} C \\ -\text{continuant} \end{bmatrix}$$

CONTEXT-SENSITIVE NATURAL PROCESSES

VOWEL NASALIZATION

$$V \rightarrow \tilde{V} \;/\; \_\_\_ \begin{bmatrix} +\text{nasal} \end{bmatrix}$$

VELAR NASAL INTRUSION

$$N \rightarrow \begin{bmatrix} +\text{back} \end{bmatrix} \;/\; V \_\_\_$$

NASAL DELETION

$$N \rightarrow \emptyset \;/\; V \_\_\_$$

NASAL ASSIMILATION

$$N \rightarrow \begin{bmatrix} N \\ @\text{pos} \end{bmatrix} \;/\; \_\_\_\_ \begin{bmatrix} C \\ @\text{pos} \end{bmatrix}$$

GLOTTAL STOP INTRUSION

$$\emptyset \rightarrow ? \;/\; V \_\_\_$$

SPIRANTIZATION

$$\begin{bmatrix} C \\ +\text{obstr} \\ -\text{cont} \end{bmatrix} \rightarrow \begin{bmatrix} C \\ +\text{cont} \end{bmatrix} \;/\; V \_\_\_ V$$

AFFRICATE FRICATIVIZATION

$$\overset{\vee}{c} \rightarrow \overset{\vee}{s} \;/\; \$ \_\_\_\_$$

OBSTRUENT VOICING

$$\begin{bmatrix} C \\ +\text{obstr} \\ -\text{voice} \end{bmatrix} \rightarrow \begin{bmatrix} C \\ +\text{voice} \end{bmatrix} \;/\; V \_\_\_ V$$

S-ASPIRATION

$$s \rightarrow h \;/\; \_\_\_\_ \begin{matrix} \$ \\ \# \end{matrix}$$

S-DELETION

$$s \rightarrow \emptyset \;/\; \_\_\_\_ \begin{matrix} \$ \\ \# \end{matrix}$$

F-ASPIRATION

$$f \rightarrow h \;/\; \_\_\_\_ \$$$

VOWEL DEVOICING

$$V \rightarrow \begin{bmatrix} -\text{voice} \end{bmatrix} \;/\; \begin{matrix} \overset{\vee}{c} \\ \overset{\vee}{s} \end{matrix} \_\_\_\_ \#$$

NON-STRIDENT OBSTRUENT DELETION

$$\begin{bmatrix} C \\ -\text{son} \\ -\text{strid} \end{bmatrix} \rightarrow \emptyset \;/\; \_\_\_\_ \#$$

APOCOPE

$$\begin{bmatrix} e \\ -\text{tense} \end{bmatrix} \rightarrow \emptyset \;/\; \begin{bmatrix} C \\ +\text{cor} \\ +\text{ant} \\ -\text{tense} \end{bmatrix} \_\_ \#$$

The context-free processes of Vowel Denasalization, Glottal Stop Deletion, Nasal Develarization, and Despirantization must be assumed to capture the generalization that all such segments are not possible lexical segments of MCS. The full defense of such paradigmatic processes as actual substitutions in adult grammars (they are well documented for child language) is to date still lacking in Stampe's work (n.b. Chapter Two above), and it is only possible to conclude that this type process is not to be discounted through any formal linguistic argument. The apparent applications of such processes in the lexicalization of loanwords at present provides their most persuasive rationale.

Figure Sixteen provides no more than a selective list (in a notation which in turn is only suggestive and not at all complete) of potential processes of interest to the current discussion. Among the context-sensitive processes, those of Velar-Nasal Intrusion, Vowel Nasalization, Nasal Deletion, Glottal Stop Intrusion, Spirantization, and Apocope will be taken up in the final section of this chapter and therefore require no further comment here.

The simplified notation used above for the process of Nasal Assimilation captures a generalization that nasals will often (though where other processes conflict, as in MCS, not always) assume the position of articulation (the cover feature "position" above) of the following obstruent consonant. The process of (Intervocalic) Obstruent Voicing seems to be a highly optional process peculiar to speakers from the Province of Havana (Saciuk 1974). With regards to Final Non-Strident Obstruent Deletion, Hammond (1976, page 143) points out that while in most American Spanish dialects the inventory of word-final phonetic consonants is limited to [n,d,r,l,s,x/h], for MCS speakers this process

has optionally become more generalized to include all word-final consonants (though while any consonant may be deleted word-finally in this dialect, no single speaker seems regularly to delete them all). Hammond (1976, page 147) also determines from his extensive work with MCS informants that the ordinarily limited process of Vowel Devoicing is also found to apply for some speakers in environments after other consonants as well.

The processes aspirating and deleting the strident obstruents prove to be considerably more problematical and raise a number of vital issues of a theoretical nature that can not be thoroughly answered within the scope of this present study.[7] First is the weighty question of whether S-Aspiration and S-Deletion should be interpreted as distinct though teleologically related substitutions or should be taken as the manifestations of a single process. And are syllable-final and word-final applications not in essence the application of a single process, where the degree of weakening involved is enhanced by the strength of the adjacent boundary (deletion being more frequent adjacent to word-boundary and aspiration more frequent adjacent to syllable-boundary -- at least according to the results of Hammond's research)?

Similar questions about the interrelations of processes and sub-processes also might be raised, of course, in regards to Spirantization versus Intervocalic Voicing of Obstruents and final consonant deletions (manifestations of weakening processes), or in relation to Vowel Nasalization versus Nasal Deletion, Nasal Velarization, and Post-Nasal Glottal Stop Intrusion (manifestations of assimilation processes). The more complex notion of such "weakening chains" or "target chains" provides the subject for study in Chapter Eight. We will return now to three particular substitutions which have proven problematical within Hammond's

discussion of the rapid speech phenomena of MCS.

5.4 Implications of Natural Processes

If realization of nasals as velar in syllable-final or utterance-final position in MCS is not an obligatory and exceptionless process, it is obviously an active one. Therefore some rule or process which corresponds to Guitart's Consonant Neutralization Rule (7) is needed within the phonological component of MCS as well as for ESH.

Yet to assume this "rule" is a traditional phonological rule with the cognitive properties Guitart assigns it proves sufficiently troublesome. First off, to adopt an SPE-type rule provides us with immediate ordering difficulties. The occurrence of standard nasal assimilation, which is also frequent before adjacent obstruents, and other assimilations (e.g. tn becomes dn in [édniko] for étnico) demands a series of assimilation rules ordered after rule (7). But then what governs when rule (7) (or for that matter other assimilation rules) applies and when it doesn't? The problem is of course alleviated when we take these substitutions to be processes (optional) and not rules (obligatory) and to apply at the conclusion of the more cognitive type rules.

A similar solution can be reached for a number of the difficulties Hammond encountered in formulating phonological "rules" to account for rapid speech substitutions in MCS. Let us restrict our discussion here to a limited number of illustrative cases: Final E-Apocope, the process of Obstruent Spirantization, and the occurrences of the velar nasal phone.

For one thing, Hammond notices certain apparent difficulties with the standard Apocope rule of Spanish (Harris 1969, Foley 1965, Saciuk 1969, etc.) as it would have to apply in light of the more restricted

(yet at the same time more numerous) inventory of word-final segments in MCS. As a result he proposes adding to the already sufficiently complex cognitive properties of a generative grammar of MCS by reinterpreting the Apocope rule as a Global Rule or derivational constraint. A more appropriate solution is to recognize the nature of Apocope as a natural morphophonemic process preceding the later allophonic processes of the grammar.

For standard American dialects of Spanish the occurring final consonants are [s,n,đ,r,l,x] and the constraint governing the surface appearance of a proposed underlying lax final vowel (all adjectives and nouns whose surface realizations do not end in an unstressed vowel being assumed to have one underlyingly) is captured by the rule given in (9) as follows:

(9) SPANISH FINAL APOCOPE RULE[8]

$$\begin{array}{l} \quad\;\; e \\ [\,-\,\text{tense}\,] \;\rightarrow\; \emptyset \;/\; V \left\{ \begin{array}{lll} \text{+coronal} & 1 & \\ [\,\text{+anterior}\,] & & \text{(a)} \\ \text{+voice} & 0 \;\_\_\_\; \# & \\ & & \\ y & & \text{(b)} \end{array} \right. \end{array}$$

Since Hammond reports the permissible final consonants of MCS to be [n,h,l,ɫ,l̥,r,ŕ,ṛ́,r̥], rule (9) is not at first impressions operative for MCS. Hammond admits that one alternative would be to simply order (9) before other processes which would then convert systematic /s,n,d,r,l/ to their various appropriate surface realizations in MCS. This appears too much like an appeal to a principle of extrinsic rule ordering which Hammond (probably with justification) is quick to reject as an overly powerful generative device. Since the environment for a new rule of Apocope in MCS would necessitate a structural description consisting

of eleven phones which do not appear to comprise any natural class, Hammond advocates replacing the structural description portion of Harris's rule with a derivational condition: i.e. the application of this rule must not violate surface structure constraints of MCS, in which the only allowable final consonants are those nine listed above.

The application of something approximating Harris's original Spanish Apocope rule (9) to MCS offers little real difficulty, on the other hand, if we assume the substitution is in fact a morphophonemic process (constraint) which is relevant at the phonemic and not at the phonetic level of representation. Once this process has applied, eliminating all systematic phonemic (underlying) final /e/'s and assuring as final consonants just those which are permitted underlyingly for MCS (which we will assume has the same underlying inventory as the standard dialects), then distinct allophonic processes will apply to the phonemic representations /s,n,d,r,l/ to produce numerous surface alternations in MCS like [h,l,$\acute{r}$,$\acute{r}$] etc. This proposal makes no appeal to arbitrary extrinsic ordering of phonological rules but rather identifies the function of a natural process as morphophonemic, which means that by definition such a process is taken to apply at the pre-phonemic level. We must also reject Hammond's insistance that the phonemic (systematic phonemic) inventory of MCS is different from that of standard Spanish. There are no empirical proofs for the nature of underlying inventories; we have only the evidence of productive processes apparent in the grammar.

This proposal has the added advantage of accounting for the existence (as actual allophonic substitutions) of what Hammond refers to as a derivational constraint (a device as powerful and unexplanatory as any principle of extrinsic ordering which he rejects) on final consonants

in MCS. The allophonic processes which would account for the phonetic realizations of final consonants would be all the following: S-Aspiration and S-Deletion; Final D-Deletion; Velar Nasal Intrusion; and processes which account for the co-articulation, devoicing, flapping, trilling, retroflection, and aspiration of the final systematic liquids. We may simply assume that /x/ (the other segment accounted for in Harris's rule) does not exist as an underlying segment for MCS.

Consider now the case of the velar nasal. Hammond objects to Guitart's thesis that syllable-final and utterance-final realizations of nasals as velar are both highly natural and support for a theory of Relative Markedness, basing his objections on the reasoning that (1) Guitart's claims about the physiological and neurological ease involved (that it is easier to have one set of neurological instructions than to have instructions involving six distinct segments) are excessively hypothetical; (2) such a claim is contradicted by typological evidence from the languages of the world (the velar nasal is relatively rare in natural languages); and (3) Guitart's analysis offers no explanations whatsoever for why nasals preceding other consonants should be realized as velars and not undergo the more standard and seemingly more natural (at least more easily explainable) standard Nasal Assimilation process. What seems most troublesome for Hammond is that before alveolar segments (e.g. [unseñór]) an actual nasal dissimilation seems unaccountably to occur.

Admittedly Guitart's account of velarization is not satisfactory nor entirely explanatory (this process doesn't seem ultimately to have much to do with a general pressure to achieve all consonants as [+back] in final position — except perhaps in the very general sense that all

these processes are in some most general way a conspiracy toward weakening phenomena); yet the appeal to physiological ease of articulation is intuitively more acceptable seemingly than Hammond's appeal to typological study. It is factual that many languages of the world display complex consonant clustering; it is also apparent that most of these languages (especially in rapid speech) display processes which aim at reducing such clusters. What is popular does not always correspond precisely to what is most natural (in any physiologically defined sense) among languages of the world.

Hammond makes additional observations concerning the occurrence of nasalization phenomena in Spanish dialects which may, when more properly assembled, hint at the reasons for nasal velarization even in unexpected (highly-marked) positions, such as before vowels and before alveolar obstruents (in which case, as reported, dissimilation seems to be taking place). Among such observations are these:

(10) The "rule" of nasal velarization does not apply in isolation but always in conjunction with an obligatory process which nasalizes vowels preceding nasal segments. The Vowel Nasalization process normally (when velarization does not apply) is followed by either nasal assimilation (of the adjacent nasal segment to some following obstruent) or nasal deletion of the following nasal segment. Also observable in MCS (cf. Hammond 1976, page 115 and following) is a process which realizes VN sequences as V? (given above in its first approximation as the Glottal Stop Intrusion process).

(11) It is a fact no longer disputed among the majority of phonologists working with Cuban Spanish and other similar dialects that nasalized vowels are often impressionistically perceived as being

followed by velar nasals. Or put differently, it is most difficult indeed acoustically to distinguish between $\tilde{V}$, $\tilde{V}\eta$, and $\tilde{V}n$.

(12) With MCS, while it is certainly not true that all closed-syllable-final and utterance-final nasals are velar (as Guitart claims for ESH), it is at the same time notable that velar nasals are being generalized so that "although, in ESH, the appearance of the velar nasal is limited to utterance-final and syllable-final environments, the surface manifestations of [ŋ] are more widespread in MCS, whereby the velar nasal may occur in almost all phonological environments, including syllable-initial within an utterance" (1976, pages 213-4).

Let me offer, then, somewhat tentatively, the following solution, based in part on observations like those in (10) through (12). I would propose that Nasal Velarization (Velar Nasal Intrusion) in ESH and MCS (and other dialects where it occurs) is but one synchronic manifestation of a larger series (a "weakening chain") of weakening and deletion processes which are operative historically in these dialects in something that resembles the subsequent progression.

(13) SPANISH NASAL WEAKENING CHAIN

vn	→	$\tilde{v}n$	→	$\tilde{v}\eta$	→	$\tilde{v}^{\eta}$	→
STEPS 1		2		3		4	

$\tilde{v}^{?}$	→	$\tilde{v}$	→	v
5		6		7

Some evidence for the final stage of this development (a complete loss of nasalization) is already attestable in rare instances, which would suggest that such a chain is at least not unreasonable. Such a process turns up sporadically on my own tapes of MCS with the single lexical item entonces ([ẽtóse] or even [etóse]) which is a word with little functional load and thus able to suffer distortion of this type with little effect on communication.

I would suggest further that what is being called Velar Nasal Intrusion is in fact a variation of a more expected assimilatory property in nasals, in this instance a progressive assimilation to the quality of the preceding nasalized vowel. Notice that while velarization of nasals occurs in MCS syllable-initially after resyllabication in forms like [u$ŋa$mi$ɣo] un amigo, nevertheless it will always occur post-vocalically. What Hammond finds odd -- the dissimilation of nasals with the occurrence of [ŋ] before alveolar segments -- is not under this analysis a dissimilation at all, but rather the case of one type of nasal assimilation winning out over another. The concept of competing processes in an identical environment as outlined in Chapter One is of course foundational to Natural Phonology. Why one process dominates and another more frequently remains unapplied in cases of competing processes is still never fully explainable, however.

There is also sufficient physiological motivation for such a natural process of progressive nasal assimilation. During the production of nasal vowels the velum is lowered to provide the most prominent obstruction in the oral cavity. However during the production of non-velar nasal consonants the velum is only a secondary obstruction, with the prominent obstruction a complete closure elsewhere in the oral cavity. By maintaining the point of closure at the velum (i.e. by assimilating the point of closure to that of the immediately-previous vowel) rather than transferring it forward in the mouth, an easier articulation of a complex sequence assuredly results.

One final consonantal phenomenon of MCS requires mention. In Hammond 1975 an argument is presented to the effect that the wide occurrence of the [+continuant] variants of voiced obstruents /b,d,g/

(viz. [ƀ,đ,ǥ]) in all phonological environments as opposed to a more limited occurrence in standard Spanish (only after other [+continuant] segments -- i.e. not after pauses, nasals, or (for d) systematic l) would motivate a possible reinterpretation of the systematic phonemic (underlying) voiced obstruents of MCS as /ƀ,đ,ǥ/. There are several additional arguments offered in support.

(14) Comparing the surface forms of MCS ([+continuant] voiced obstruents in all environments) with those of standard Spanish, the trend of phonological change is clearly in the direction of continuancy. This directionality of change would seem to suggest a need for the restructuring of synchronic underlying forms to correspond to the surface realizations.

(15) The assumption of underlying [-continuant] obstruents would entail accounting for the change in MCS by proposing the spreading of a rule of Spirantization. The assumption of underlying [+continuant] obstruents would allow us to hypothesize the loss of a rule of Despirantization (which rule would presumably have been earlier applying in those environments where MCS still had surface [b,d,g]. Since it might be assumed to be a tenet of generative historical linguistics that rule loss in specific environments is less marked than rule spread to new environments (Hammond 1975, page 6), such a criterion might here be applied to support the notion of restructuring.

(16) Since in MCS spirantized obstruents are far more frequent, proposing these segments as underlying would allow the elimination of a phonological rule of Spirantization, and this reduction in rules would seem attractive in a grammar built on a notion of rule economy.

The questions raised by Hammond's paper are then (1) what motivates underlying stops in this circumstance rather than the underlying spir-

ants? and (2) what motivates positing a rule of Spirantization (which Harris 1969 does not sufficiently motivate either) versus a hypothetical rule of Despirantization?

The most direct answers to these questions would seem to come from the realm of loanword phonology, and again it would be best to defer a fuller discussion to Chapter Seven. Yet it can be noted at this juncture that there is a strong naturalness argument surrounding the rule (or process) of Spirantization. A number of the borrowings of MCS cited in Chapter Seven and Appendix Part III reveal a regular application of this productive allophonic process in the native Spanish pronunciations of foreign lexical items. In treating borrowings, however, the MCS speaker displays no evidence of any active process of Despirantization. Further, if there is a Despirantization rule it would be one that would have little motivation physiologically or on the basis of any independently attested alternations and a strong outside motivation (empirical evidence) for positing such a rule would seem mandatory.

The problem with claiming that spirants might now occur in the MCS lexicon is that we would then be denying in effect the existence of Spirantization as a real and active process of the grammar, despite its strong motivation found in, say, loanwords. Also, since the reported spread of the surface spirants is noticeably related to rapid speech phenomena, we would be denying as well that what is actually transpiring is the spread of a process motivated by pressures in the direction of articulatory ease; we would be bordering instead on the absurd claim that what fast speech actually conditions is change in the lexicon.

In short, Hammond (1975) provides two minor arguments (those in

(15) and (16) above) for spirantized voiced obstruents in the lexicon and ignores the most evident and compelling argument bearing on this issue. Since we never have more than the most indirect kinds of evidence for what constitutes a lexicon, while we do have tangible evidence for the reality of attested processes, it would seem best to base any arguments on the evidence for Spirantization, rather than on vacuous notions about symmetry and markedness which comprise Hammond's minor arguments.

If a present trend remains in evidence and [+continuant] voiced obstruents eventually become unexceptional in all environments on the surface, then it might appear that we have a case of Kiparsky's notion of absolute neutralization (Kiparsky 1968a) and again a motive for hypothesizing restructuring. Yet there is no real motivation for any assumption that the trend is toward continuancy in all environments -- but rather only in those environments where it is explained by natural processes. Also, Kiparsky's Alternation Condition which is relevant at the traditional phonemic level (for discussion of this point see e.g. Rhodes 1974) and thus unaffected by purely allophonic processes would not in any way be violated here:

(17) THE ALTERNATION CONDITION AND ORDERING OF PROCESS-TYPES

d systematic phonemic representation

d phonemic representation (some intermediate level which is the level at which the Alternation Condition is determined)

SPIRANTIZATION (independently evidenced by loanwords)

ɸ surface phonetic form

Rapid speech demands weaker articulations. What is happening to

voiced obstruents in MCS would seem to tell us something about what processes are applying, not about what is established in the lexicon. Is it the environments where these segments are found that are on the increase per se? Or is a process spreading to new environments? There is of course a major descriptive and explanatory difference. Are we to suggest that speech styles (rapid speech versus formal speech) will determine the lexicon, or that there is more than one lexicon for each and every speaker, dependent on the care and speed with which he speaks?

Finally, as one additional shread of evidence that what we have involved here is the active operation of an innate process, and not any form of lexical restructuring or manipulating of underlying forms, note that this change as we have described it for MCS occurs only with the voiced obstruents. This is one further indication that what is involved here is a conspiratorial "weakening chain" of a type discussed more thoroughly in Chapter Eight.

NOTES

[1]It is not being claimed here that Miami Spanish is in fact a single and unified dialect, though it is not necessary, at the same time, to assume a hopeless conglomerate of Spanish dialects from the wide range of Latin American countries. Quite obviously, to be sure, the Spanish-speaking population of Miami (or Greater Dade County) is not exclusively Cuban; also, among the Cuban population itself all six provinces and therefore a number of distinct original dialects are represented. On the other hand, the population of Spanish surnamed in Greater Miami is heavily Cuban and the original dominant dialect (preceding the exodus from Cuba) was the standard dialect of Havana (which we might assume to be something like that characterized by Guitart as the Educated Spanish of Havana). There is no entirely accurate recent census, yet the 1970 census figures show the number of Cuban residents in Dade County to be approximately 218,000 or close to a fifth of that county's total population; conservative estimates would now place the current number at above 600,000 or over fifty per cent of the total population of Dade County. What is most important here (linguistically) is that there are in evidence features (the degree of velarization in final nasals, e.g., or the intervocalic voicing of voiceless stops observed by Saciuk 1974) which distinguish the phonology of Miami Cuban Spanish from the Spanish of other non-Cuban Carribean dialects (though not perhaps from the dialects of insular Cuba -- note e.g. that the voicing phenomenon discussed by Saciuk is apparently a characteristic of certain speakers of Havana Spanish and not inherent to the Miami dialect). After the fashion of Hammond (1976), then, and in order to facilitate discussion (since my purpose here is not finally to delineate the Miami dialect), we will carry on the convenient fiction that the Spanish of Miami Cubans represents a single unified and cohesive dialect, in the same sense as Educated Spanish of Mexico City or Educated Spanish of Havana. There will, of course, be many close parallels and shared features with the latter.

[2]While the substitution of Velar Softening can be taken as most classically a learned phonological rule in English, applying to limited sets of formatives (those of Romance origin), easily suspended by a conscious effort of the speaker, and therefore representing no real constraint on pronounceability (see Section 2.2.1), Velar Softening, at first appearances, might be taken among the natural processes of Spanish dialects. Harris has demonstrated (1969, page 164 and following) that the so-called Velar Softening process of Spanish is in fact a number of distinct diachronic processes represented quite loosely as

$$k \rightarrow t^{s} \rightarrow (d^{z} \rightarrow z \rightarrow) s$$
$$g \rightarrow \overset{\vee}{j} \rightarrow \overset{\vee}{z} \rightarrow \overset{\vee}{s} \rightarrow x$$

The full discussion of the justification of such a string of historical processes is given in Harris (pages 163-73). Intermediate derivational stages of this type are one feature that would distinguish the true processes from rules. What must not pass unnoticed, however, is that Velar Softening for Spanish is replete with numerous exceptions (Harris 1969, pages 173-7). Harris accounts for some of these exceptions in verb forms (e.g. the indicative pa[g]o and subjunctive pa[g]e -- not *pa[x]e -- of pagar etc.) by an underlying and subsequently deleted theme vowel following /g/ at early stages of the derivation (1969, pages 71-2); and other exceptions (e.g. á[x]il "agile" versus á[g]ila "eagle" or [x]itano "gypsy" versus [g]itarra "guitar") are taken to affirm that forms with [x] and [s] have systematic phonemic /g/ and /k/ while those with [g] and [k] must have /g^w/ and /k^w/. However little in the way of explanatory value may be offered by such manipulations of Spanish phonological representations, these facts are some clear indication that the rule involved is certainly not a restriction on the pronounceability of /ki/, /ke/, /gi/, /ge/ sequences for the speakers of Spanish. We can only conclude that Velar Softening in Spanish must have a learned abstract environment and not a physiologically restricted environment and therefore must be a phonological rule and not a phonological process of these dialects.

[3]Another Spanish dialect displaying similar notable application of nasal velarization (and even the further nasal deletion discussed below) is the dialect of Dominican Spanish reported on by Jiménez Sabater (1975). E.g. (with the sequence nm if forms like conmigo)

> Por mi parte, pude percibir en el país cuatro soluciones diferentes que son, en orden de mayor frecuencia, las siguientes: una asimilación a la bilabial siguiente [kommígo] o [ko^{m}mígo]; una disimilación de la nasal alveolar, que se convierte en oral y se manifiesta, según las regiones, con las formas [koɪmígo], [ko^{l}mígo] o [koimígo]; una pérdida de la /n/ con nasalizacion de la /o/ precedente; un alófono velar [koŋmígo]. Por lo común alternaron varios resultados en una misma localidad (page 114).

[4]The following data from my own tapes is illustrative here first of the treatment of nasals by representative speakers of MCS.

CITATION FORMS	INFORMANT DC	INFORMANT AF	NUMBER OF INFORMANTS WITH	
			VEL. NAS.	NAS. ASSIM.
un boleto	[m]	[m]	1	11
un francés	[m̩]	[n]	2	10
un domingo	[n̩]	[ŋ]	1	11
un señor	[n]	[ŋ]	4	8
un chiste	[ń]	[ń]	3	9

Informant DC is 24 years old, female, and largely bilingual while Informant AF is 48 years old, male, and monolingual (Appendix Part I)

[5]Guitart's work is purportedly designed to salvage an original SPE concept of "markedness" theory in generative grammar by replacing Chomsky and Halle's absolute markedness values with a theory of Relative Markedness. Guitart's theory is designed to overcome the empirically unjustifiable notion that marked segments are more complex and the unmarked segments less complex in every respect. Guitart observes that "In Chomsky and Halle (1968) the terms 'Marked' and 'Unmarked' do not relate to any empirical facts; rather, they refer to what complicates or does not complicate a grammar" (1976, page 59). Yet "the main proponents of Markedness have ignored the possibility that these criteria may be in conflict, i.e. that the U value may be more complex than the M value on grounds other than those on which the assignment was made" (page 60). Guitart's solution is to propose a grammar sensitive to both physiological and perceptual markedness conditions. Like the proponents of the original SPE notion, however, Guitart offers no evidence that "marked" and "unmarked" relate to any empirical facts -- that markedness "conditions" are any more than the active syntagmatic and paradigmatic processes for which the grammar must already account. Without such evidence, further speculations about revisions within a formal theory of Markedness are at very best unproductive and at worst altogether misguided.

[6]Perhaps the most unreasonable and unsupportable of the several claims Guitart makes about consonantal phenomena in Havana Cuban Spanish is that involving s-deletion and found in Guitart 1974:

> Aspiration in Spanish is not the only mechanism available to the speaker who does not need to realize /s/ as an anterior coronal strident in preconsonantal position. There is also deletion, as in [éto] for esto "this", [mímo] for mismo "same", heard in Black Cuban (1974, page 4, with my italics -- PCB).

The notion that such deletions are indicative of lower-class Black dialect and do not show up among educated upper-class speakers of the Havana dialect (see also Guitart 1976, pages 78-9) is nowise consistent with my own observations (nor those by Hammond) of Miami Cuban informants, many of whom are well-educated natives and long-time residents of Havana Province. Among my own informants, for example, DC (24, female, graduate law student, born in Havana and resident there 17 years, receiving her pre-university education in that city) and AV (37, male, newspaper reporter, graduate of Cuban university, born in Havana and resident there 23 years) both displayed frequent but yet random aspiration and deletion of syllable-final s, throughout their interview sessions.

[7]One issue that will be raised here is the status of S-Aspiration as an allophonic or morphophonemic process. Although this process (particularly in dialects where it applies syllable-initially) seems to neutralize an underlying distinction (derived [h] and non-derived [h]) and thus qualify as morphophonemic in effect, yet its widespread application in loanword phonology (where allophonic processes are exclusively relevant) seems, on the other hand, to belie such a status. In effect, S-Aspiration in MCS provides a considerable impetus to the demands for a revision of Stampe's original notion of allophonic versus

morphophonemic. This question will receive treatment in Chapter Seven.

[8]The rule here is as given in Harris 1969 (page 177). For a formal defense of the final lax /e/ as an "abstract" underlying segment in the grammar of Spanish, see especially Harris 1969, page 177 and following.

CHAPTER SIX

LOANWORDS AND PHONOLOGICAL STRUCTURES

> Spacially and temporally, borrowing is an extremely complex phenomenon, for it involves the transmission at various times, through various dialects of both the source and target languages, through various intermediate linguistic communities, and by various speakers (or groups of speakers) of all these dialects and languages, of semantically disparate lexical items. Each of these complicating factors can phonetically distort the "original" form of the word and thereby render incorrect any conclusions about adaptation or adoption which are based solely on the presumed first and last stages of the borrowing process.
>
> -- Kyril T. Holden

6.1 Structural Implications of Borrowing

A plethora of literature exists on questions of linguistic borrowing and most of the issues involving loanwords have been quite well defined even though rarely resolved.[1] That so little like real progress has been made in deciphering the factors which determine how languages in contact will interact, and what principles will govern the rates and directions in phonological assimilations of foreign elements, is perhaps most directly attributable to the multiplicity of factors which are ultimately involved.

Holden (1972) has provided a thoroughgoing study of some of these difficulties as they confront the phonologist encountering loanword phenomena, and a comprehensive survey of loanword problems may be deduced from Holden's overall treatment (the features marked with the symbol * being those of special relevance to this present study).

(1) Since borrowing often involves the transmission of unrelated

lexical items through various dialects in both the source and target language -- at various times and by various speakers of differing degrees of bilingualism -- a number of evasive factors may render nearly unattainable any firm decisions about the history of adaptation of a given form, and at the same time totally obviate any decisions based solely on the first and last stages of this borrowing process.

(2) Borrowing may be either Direct, from Language A to Language B without any intervening stages, or Indirect, in which case a borrowed form passes through intermediate languages or dialects and enters the target language from some secondary source that has almost assuredly already rendered it into a new phonetic form distinct from the original.

(3) In more highly literate societies, where some degree of bilingualism is found, words are often borrowed via printed matter rather than through their spoken forms, and when this occurs modification of phonological words is often not a product of the operative phonological processes so much as it is of the prevalent conventions for transliteration from one orthographic system to another.

*(4) Even in those rarer cases of direct phonetic borrowing, the monolinguals of the community are dependent in most cases for their perceptions of new lexical items entering through borrowing on initial carriers of the foreign words (who are the bilinguals of the community), and here the difficult-to-capture variables which may affect later perceptions and renditions of the borrowed forms by the monolingual community are both (a) the varying manners in which bilinguals attempt to articulate these foreign forms, since distortions passed on the the monolinguals may not result from the influences

of the target system per se but rather from attempts of the bilingual carriers to sound "foreign" or from their incorrect attempts due to faulty knowledge of the source language, and (b) the imposition of artificial and incorrect foreign language pronunciation conventions in the community schools, where these bilinguals normally have obtained their functional knowledge of the second language.

*(5) Even where there are no "artificial" factors such as those cited above to distort the phonetic shape of the borrowed form, we may still expect foreign features to be preserved longer among bilingual or partially bilingual borrowers than among their monolingual counterparts, and this means at best that foreign forms will be nativized at different rates in different segments of the borrowing population.

*(6) Just as certain orthographic conventions for borrowings may become established, it is also poosible that monolingual speakers of a target language, on the basis of encounters with a large number of similar forms, will adopt a certain pattern for adapting peculiar features or segments that is only indirectly related to a regular phonological adaptation of such segments within that target system.

*(7) Certain foreign words or, more likely, certain classes of words, often receive a special treatment by the monolingual speakers of a target language and the assimilations of such words (e.g. proper names) may well be quite different from that of other words being borrowed from the same source language during the same period of time.

(8) If borrowing does not occur during a single period of restricted time (as usually it does not), then a possible compli-

cating phenomenon of secondary borrowing may arise, whereby a second borrowing of the same word from the same source language amy occur after a given period of time -- and quite expectedly (due to sound changes occurring in either system) this second assimilation may be markedly different from the first.

(9) Related to this issue of secondary borrowing is also a problem which arises from the fact that either the target or source languages may have undergone historical sound changes in the interim -- when borrowing takes place over a considerable period of time -- and these changes again make it exceedingly difficult to draw any firm conclusions about the original phonetic shape of the borrowed word as it first entered the target system, or about the types of constraining rules or processes to which it may have been exposed along the way.

*(10) It is also apparent that different segments may behave quite differently with regards to a particular constraint in the system of the target language -- that is, they may have different rates of assimilation and these rates may vary not only among the different foreign segments themselves but also among the different constraints in the target system to which the individual segments and sequences of segments are subjected

(11) Finally, it is possible to find cases involving secondary borrowing in which the source form of the new adaptation of an earlier borrowed word comes from an entirely new language, rather than simply a different historical stage of the same source language, and failure to uncover this fact may again lead to serious false assumptions about the behavior of certain loanwords during the course of their

complete assimilation.

(12) In general, it is usually quite impossible to be certain how or by whom any particular loanwords were first introduced into a chosen target language, and therefore to determine one of the most essential pieces of data for any study of the adaptation of borrowings -- i.e. what precise phonetic features were the special characteristics of the form when first borrowed and what distortions of these were made by the very first (presumably) bilingual borrowers?

Such factors, along with others that might be listed, underscore the frequent inscrutability as well as the potential value and fascination of loanword data. Most of the above features will not be altogether relevant to the present treatise on borrowing and therefore need not trouble us further in this dissertation.

We will explore here only the kind of direct borrowing which we will label direct phonetic borrowing, in which bilingual and often even monolingual speakers are exposed first-hand to pronunciations of the borrowed forms in the original source language by the native speakers of the source language. What we encounter here, then, is a perception and reproduction (or attempted reproduction) of the acoustical signals of the foreign form by monolingual speakers of the target language, in which case this perception and reproduction is a direct product of the internalized grammars of these borrowing speakers.[2] The problems identified by features of borrowing labelled (6), (7), and (10) (and sometimes (4) and (5) when monolinguals have adopted pronunciations from their bilingual counterparts) are, in this case, all that remain relevant to the type of study we have in view.

It is a raison d'etre of this dissertation that borrowing of this

direct phonetic sort holds considerable significance for the general study of phonology at large, since it is inevitable that observations possible about perception and rendition of foreign segments are in effect observations about (and even evidence for) the types of grammars possessed by bilingual and monolingual speakers engaged in the productive act of borrowing. Elsewhere, it is only in child language that we can claim an identical direct access to what processes are actually productive for the native system; and child language, after all, can only idirectly reflect those processes which are maintained in the adult grammatical system. Loanword phonology is, then, nowise limited to activity fascinating and challenging but unproductive of theoretical significance. Rather, it is to be understood as vital to our understanding of the nature of all synchronic as well as diachronic grammatical structure.[3]

6.2 Earlier Inadequate Approaches to Borrowing

Major landmarks in the investigation of loanword phonology have been the earliest Structuralist studies in the modern era by Haugen and Weinreich, the "Admissibility Theory" superficially sketched by Chomsky and Halle in The Sound Pattern of English and accepted as the standard explanation for phonological adoption by proponents of generative phonology, the recent "Magnetic Attraction" Hypothesis concerning relative rates of assimilation propounded by Holden (1976), and the Natural Loanword Phonology of students of Stampe. The remainder of this chapter will constitute a brief survey of each among these developments, culminating with a suggestion that the Natural Phonology of Stampe provides several answers unattainable through

the earlier theories. This last point is best illustrated by a brief demonstration that although the several approaches of Hyman, Harms, Kiparsky, and Vennemann in the generative camp are not able to resolve the controversial Nupe borrowing problem introduced in Hyman 1970a and Hyman 1970b, the Natural Phonology treatment (first suggested in Ohso 1971) does provide considerable insight along with elements of explanation.

6.2.1 Haugen and Weinreich and Structuralism

Structuralist studies like Haugen 1950 and Weinreich 1963 approach examples of phonological interference in borrowing situations through what might be conveniently labelled the phonemic approximation theory of loanwords. The most basic and common activity of linguistic borrowing is taken to arise when a speaker employs a native sound sequence to imitate a foreign one (Haugen 1950); and the linguist must rely on syntagmatic factors (temporally ordered sequences of sound) and paradigmatic factors (fixed positions in the chain of speech) as a basis for his description of this event (Weinreich 1963). In light of Weinreich's (1963, pages 18-9) typological scheme for paradigmatic interference, the speaker of the borrowing language may be viewed from within this frame as performing the following potential types of linguistic behavior:

The Under-Differentiation of Phonemes occurs when two sounds of source language system whose closest counterparts are not held to be different in the native language system are confused.

The Over-Differentiation of Phonemes is the imposing of the phonemic distinctions of the native language system on the sounds from

the source language system, precisely where this is not required. The Reinterpretation of Distinctions comes about when bilingual speakers distinguish phonemes of the source language system by the features which in that system are merely redundant, but which in his own system of native phonemes are perfectly relevant. The Actual Substitution of Phonemes applies to only those phonemes which are identically defined for the two language systems but whose normal pronunciations are different.

The insufficiency of this approach is most notably its failure to consider adequately the role of phonetic cues operative in phonic interference. One necessary condition of linguistic interference was taken to be what Weinreich (1963, page 7) terms interlingual identification and which is the perception of equivalences between two languages based on similarities in shape and distribution. But the issue remaining unresolved was how exactly the native speaker was to arrive at these equivalences. And another difficulty was inevitably the nature of the taxonomic phonemic approach to linguistics itself. Since this was in practice a descriptive theory, it offered little if anything about what would happen in the actual language-contact situation. There was no way to get at how a speaker would actually map one linguistic system onto another.

The earliest even moderately successful linguistic explanation for phonic interference phenomena in borrowing situations was what is popularly known as the phonetic approximation theory. That this phonetic approximation theory has always occupied a somewhat ambiguous position in the science of loanword phonology is observed by Lovins in the following terms: "This concept is clearly not sufficient to

account for all phonological interference data, but neither is it totally obsolete, in that there are well-documented cases for which no satisfactory explanation has been offered in terms of anything more abstruse than low-level phonetic detail" (1973, page 16). The Waterloo for the phonetic approximation theory is obviously reached in the face of numerous extant cases where members of the target language phonetic inventory are identical to counterparts in the secondary source language and yet an entirely different substitution is still made. Some more abstract aspect of phonological structure seems to be involved in these cases and Lovins (1973, page 19), for one, suggests a number of plausible types: differences in allophonic distribution; differing sequential restrictions at some level other than allophonics; stylistic factors in the phonology which lead to only marginal equivalences of corresponding phonemes.

Hyman (1970b) rejects both phonemic approximations and phonetic approximations as accounts of borrowing and proposes that the only insightful account is one appealing to the role of sequentally ordered phonological rules, in the fashion of generative phonology. In rejecting both types of Structuralist approaches, at the same time he acknowledges certain slight advantages in a phonemic-based theory and even quotes Haugen to this point:

> Neither the speaker himself nor the linguist who studies his behavior is always certain as to just what sound in his native tongue is most nearly related to the model. Only a complete analysis of the sound system and the sequences in which sounds appear could give us grounds for predicting which sounds a speaker would be likely to substitute in each case (1950, page 215).

The Structuralist phonemic approach shared to a large extent, then, the feeling of Haugen that loanword phonologists must look to

obtuse features in the native phonology to find adequate explanations for borrowing phenomena. In the final analysis, however, Structuralist approaches to borrowing (and to most other questions in linguistic theory) fail essentially because they can't provide a comprehensive enough model capturing the inevitable interplay between phonemic (underlying) and phonetic (superficial) factors and accounting for constraints in both the target and source systems.

6.2.2 Chomsky and Halle's Admissibility Theory

Chomsky and Halle pay little attention with their seminal work on generative phonology to the treatment of "non-native" or labelled "exceptional" elements of the phonology. The standard theory does allow for a formal mechanism of "lexical features" (SPE, page 373 and following) which provides that some formatives are given a "lexical mark" which denotes that they are not fully nativized forms and thus stand as exceptions to the application of certain native rules.

This aspect of generative theory achieves perhaps its most thorough discussion in Saciuk's (1969) dissertation on native and non-native lexical elements of Ibero-Romance (recall Section 1.3). This notion of lexical "strata" bears disturbingly little clear correlation to any conception of "degree of assimilation" which is an inescapable concern of loanword analyses. Nor does it have any more bearing than the earliest Structuralist studies on how speakers actually go about treating the foreign features that attempts at borrowing introduce into their grammars. The failure of generative phonology to make a distinction basic in Natural Phonology between cognitive rules and innate physiological processes obviously precludes for

the generative model any account of borrowing and lexicalization of the type contemplated in this and subsequent chapters.

This notion of "degree of admissibility" of a lexical item receives only slightly more formal treatment from Chomsky and Halle (SPE, pages 416-8) and again in establishing a formalization for the concept of "distance from the lexicon" they offer little that is convertible to "degree of assimilation" or "degree of nativization."

The theory of admissibily offered by Chomsky and Halle may be collapsed and reduced somewhat for our purposes here.

> Given a lexicon L, which determines a set of interpreted lexical matrices.....we can define the "distance" from L of a lexical matrix u in the following way. Let us say that rule (16) distinguishes u from L if (16) does not change any member of L..... but (16) does change u; and furthermore, (16) is minimal in that any other rule meeting these conditions contains at least as many features F specified [+F] or [-F] as does (16):
> (16) $X \rightarrow Y / Z \_\_\_ W$
> Let us define the distance u from L as 1/n, where n is the number of features specified as [+F] or [-F] in a rule that distinguishes u from L in this sense. Where the distance is undefined in this way (there being no such rule), let us say that it is zero.
> For example, let L be the lexicon of English and consider the distance of /brik/, /blik/, /bnik/, /bnzk/ from L. The distance of /brik/ from L is zero since there is no rule distinguishing it from L. The distance of /blik/ from L is 1/17 since the minimal rule distinguishing /blik/ from L is (17), which has 17 specified features. The distance of /bnik/ from L is 1/5, since the distinguishing rule is (18). The distance of /bnzk/ from L is 1/4 since the distinguishing rule is (19).....In short, to determine the distance of a matrix u from L, we find the simpiest rule which is "true of L," in the obvious sense, but not true of u, and we take the distance of u from L to be inversely related to the complexity of this rule (SPE, pages 416-8).

Here the "distance" or "foreignness" of a word from the native lexicon is taken as a direct corollary of the feature count of some possible rule relating that word to an acceptable form, and as with the SPE notion of a simplicity metric the criterion again becomes one of feature counting. But is there any empirical evidence of

a sort that would verify that speakers in any sense judge "foreignness" in this way? And is there any correlation between rules of this type used to judge "distance" and the "rules" the speaker would apply to a borrowed form to reduce its degree of exceptionality?

If the analysis of borrowings presented in Chapter Seven is at all correct, then any theory of "admissibility" of the type presented in SPE must be dismissed as having no even remote resemblance to the apparent mechanism by which speakers will actually judge "the degree of admissibility" when they encounter a non-native form. Consider here only the following minimal list of conclusions which can be abstracted from the loanword data explored in Chapter Seven.

(12) Loanwords have their underlying representations at the phonemic level and are hence not lexicalized in the more abstract form which would be necessary to correspond to the SPE designation of systematic phonemic or lexical matrix.

(13) Speakers judge the "nativeness" of a foreign word in terms of the correlation apparent between the perceived phonetic form of the foreignism and what are acceptable phonemic forms in the native system.

(14) Perception of the "closest sound" in a foreign language takes place in terms of phonological processes, not in terms of binary features which have little support as perceptual primes.[4]

(15) The degree of "nativization" or "naturalization" of any loanword is solely a function of what processes have applied in its lexicalization.

(16) If a loanword is judged inadmissible in the native system (i.e. it can be recognized as neither an acceptable underlying

form or an acceptable derived form, resulting from application of allophonic processes), it will then be lexicalized in its foreign (perceived) shape and the context-free processes will apply to nativize it.

Each of these proposals is in direct conflict with the notion of phonological "admissibility" being offered by Chomsky and Halle in SPE Chapter Nine. A number of advantages in the Natural Phonology analysis over the generative phonology analysis of lexicalization once again loom forth. The loanword hypotheses outlined here in (13) through (16) are both explanatory of the actual strategies chosen by speakers and at the same time susceptible to testing in light of the loanword data at hand. The theory of SPE Chapter Nine is merely descriptive and offers little hope of any empirical verification.

6.2.3 The "Magnetic Attraction" Hypothesis

Holden (1976, pages 131-2) remarks that linguistic literature offers all three following positions as possibilities for describing the role and status of loanwords in a phonological system.

(17) HYPOTHESIS I would be that loanwords are completely independent from the native vocabulary, having their own set of phonemes and their own set of rules converting them into phonetic forms.

(18) HYPOTHESIS II claims that loanwords from an identical source display (uniformly and predictably) the same identical set of phonological peculiarities. It is this second assumption which generally supports the treat of SPE, where a block of "unassimilated" loanwords are provided in the lexicon with a

"diacritic feature" to trigger or block the applications of the native phonological rules. Here, particular "foreign" diacritic features with sets of special native rules to be blocked for certain borrowed lexical items, or with sets of special native rules to be triggered.

(19) HYPOTHESIS III, which is a necessary revision of HYPOTHESIS II, allows that while fixed features distinguish the borrowings within any given language these features will not be predictable for given borrowings due to the fact that individual borrowings will inevitably display different degrees of assimilation to the native system.

A corollary of this final loanword hypothesis given as (19) would be that the frequency with which a given loanword is used would show some relationship to its degree of assimilation, i.e. that a recent and relatively unfamiliar borrowing will be less likely to undergo the full set of native rules necessary for its assimilation, or put slightly differently, it will continue to display a greater evidence of those features (for Holden's Russian examples these would be e.g. (a) the existence of otherwise non-occurring phonemic f, (b) non-palatalization of consonants before e, and (c) non-reduction of unstressed o and e before certain front and central vowels) which mark it as foreign (see Holden 1976, pages 131-2).

The failure of HYPOTHESIS II in its original form is quite obviously that it makes no allowance for the needed distinction between the un-assimilated loans, partially assimilated loans, and fully assimilated loans. The SPE practice of marking all segments of an identifiable loanword with the diacritic [+foreign] proves intolerable in the light

of partially assimilated loans for which some segments fail to undergo native constraints and others don't. The modification of HYPOTHESIS II in the form of HYPOTHESIS III attempts to allow for this phenomenon of partial assimilation.

Holden is then able to report on a rather startling discovery about the "rates" of assimilation attested at least in the case of his own analysis of Russian borrowings (based on data gathered by M. Glovinskaja from word-lists selected for certain foreign features and presented for pronunciation to a group of forty selected Muscovites):

> Tentative observations of the process of assimilation of borrowings indicate that distinctive segments assimilate to various native phonological constraints at different rates. This must be a reflection of the nature and strength of the target constraints themselves, simply by exclusion: there is nothing else to account for such facts. For want of a better name, I would like to label this the Magnetic Attraction Hypothesis. The analogy implied is that each phonological constraint of the target system exerts a "magnetic" pull on the appropriate segments of the borrowing, in order to assimilate those segments to the native system. Different constraints have different strengths of attraction, and the "magnetic force" of each constraint acts differently on different segments which satisfy the structural conditions of that constraint (as though different metals were being attracted by the magnet). I further hypothesize that these varying strengths are a direct measure of the relative "productivity" or viability of phonological rules or constraints in the native system, and provide us with a glimpse of how such rules begin to decay "from within", so to speak (1976, page 133, with my italics -- PCB).

The "Magnetic Attraction" Hypothesis is a significant advancement over the concepts of "degree of admissibility" and "degree of assimilation" and provides a theory compatable with experimentation and statistical verification. Holden extends this theory, in the light of more extensive Russian data, to incorporate a further thesis that situations prevail where "the targets of various foreign features in the same word conflict with one another, as opposed to those situations in which only one target is possible with no conflicting pressure from

elsewhere in the loanword" (page 134). Holden is able to deduce in this light an approximate "hierarchy of strengths" among native constraints (i.e. processes) and assumes for the Russian data under analysis that word-level constraints (segment harmony) override syllable-level constraints (assimilations in palatalization and voicing), while the syllable constraints in turn take precedence over segment-level replacements.

These combined theories lend organization and direction to our own discussion of much of the Cuban loanword data presented in the proximate chapter, as well as providing a number of obvious theorems for testing. It was considered above in Chapter Five, for example, that both Spirantization and Velar Nasal Intrusion are active native processes of Miami Cuban Spanish with varying degrees of relative strength within the native phonological component. In view of the "Magnetic Attraction" Hypothesis we are now able to speculate that the relative strengths or productivities of these processes within the native phonology (Hammond's percentages of occurrence providing a rough guide) will be reflected in the degrees to which each is evidenced among loanword pronunciations. And it will also prove significant to our analysis of the native system to observe which processes appear to dominate with any regularity when speakers are faced in the same word with alternative strategies of analysis. One argument to be advanced in Chapter Seven is that it is data of precisely this kind which best supports the several objections raised in Chapter Five against speculations by Hammond and Guitart about the content and structure of phonological rules and phonological inventories in Miami Cuban Spanish and in Havana Spanish as well.

6.3 The Applications of Natural Loanword Phonology

In view of arguments comprising previous sections of this chapter, it would seem imperative that we abandon a phonemic approximation approach and phonetic approximation approach, or a generative approach to loanwords based on the notions of "lexical feature" and "degree of admissibility". The theory which we will adopt and submit to further analysis here is that based on the proposals of David Stampe and so far applied to the loanword phonology of Japanese by Julie Lovins (1973, 1974b) and Mieko Ohso (1971). Since this theory is in large part an outgrowth of three principles of borrowing first introduced in Hyman's studies of Nupe (cf. Hyman 1970a, 1970b), it is mandatory that we also examine briefly Hyman's original theory of borrowing.

Hyman's principles of borrowing, incorporated with modifications into the loanword theories of Lovins and Ohso, are captured in (20) through (22).

(20) HYMAN'S PRINCIPLE I: Foreign sounds are perceived in terms of underlying forms in the native phonological system.

(21) HYMAN'S PRINCIPLE II: Foreign segments which seem to be equivalent to those native segments which have been derived by rule are thus lexicalized as the corresponding native underlying forms (and then subjected to the native phonological rules).

(22) HYMAN'S PRINCIPLE III: When the foreign segment appears in some environment in which an equivalent native derived segment can not appear, there will be a modification of the form of the incoming foreign word so that the structural description of the rule is met and the borrowed segment can be properly derived in the appropriate environment.

Principles I and II can be adopted in tact by the natural loanword phonologists with one small (although significant) adjustment: underlying form is taken here to mean Stampe's notion of a "phonemic" representation (as discussed in earlier chapters) and "rule" is taken here to mean the exclusively allophonic processes which alone apply to loanwords (at least in the early stages of nativization).

Principle III, by contrast, is deemed too strong, for as Ohso (1971, page 2) observes, it provides no principled method of choosing between a number of alternative solutions. It a language with the process "A → B / ___ C" borrows the sequence BD, and yet within this language B may occur before not only C but also X, Y, or Z, on what basis will the decision be made as to what D should be changed to in order to provide a proper environment for deriving B? Ohso gives evidence of exactly such a case in Japanese where Principle III thus provides no unique solution.

Ohso proceeds to demonstrate that Principle II is in fact not actually needed to handle the Nupe example for which it was proposed. Ohso's argument runs approximately as follows. Hyman had observed that when borrowing the foreign phonetic sequence [sú] the target output for Nupe speakers was always [s^{y}i]. Since Nupe possesses a process attested elsewhere which changes unrounded front vowels into rounded back ones after labialized consonants and rounded back ones into unrounded front ones after palatalized consonants, Hyman assumed this process was applied to accomplish the change dictated by Principle III. Ohso suggests that Nupe may in fact have as well a process which assimilates features of a vowel to those of a preceding consonant (VAR), which would permit the speaker to make the following

analysis. (I will use the notation adopted for (23) both here and in Chapter Seven, after the model of Ohso, where upward arrows indicate the analysis of foreign segments as underlying representations and the downward arrows suggest the nativizing processes in the native language system.)

(23) NUPE TREATMENT OF FOREIGN [śu]

[śu] ↑ /śu/ ↓ /śi/ ↓ /si/ ↓ śi ↓ [ś^{y}i]

VAR DR SP GA

restructuring
of underlying
representation

VAR = Vowel Assimilation Rule
DR = Depalatalization-Delabialization Rule
SP = Spirant Palatalization Rule
GA = General Assimilation

After all this, Ohso is still left with the feeling that certain loanword data from Japanese demands that some (perhaps weaker) version of Hyman's Principle III be retained. Lovins (1974b, pages 245-6) shows in more sophisticated fashion that such a principle is indeed vital to loanword analysis in Japanese and can be reformulation as the following: "When a sequence of foreign sounds not corresponding to a legal derived sequence in the target language is interpreted according to a context-sensitive process, the environment of this process will be generalized to the smallest natural class that encompasses the corresponding segment in the source sequence" (246). The point is that such generalization does not occur at random (alleviating the paradox discussed by Ohso) and to some extent at least the direction this extension will follow is predictable (see Lovins's example of this in Lovins 1974b, page 245). An example of the application of a

revised version of Principle III to a specific borrowing case will be taken up in Chapter Seven and I will therefore not pursue this matter any further at the moment for the purposes of this present chapter.

Hyman's treatment of Nupe has received recent notoreity (especially as developed in Hyman 1970b) to the extent that he has employed his loanword data in the attempt to justify an abstract solution to Nupe phonology. As this article has received such wide treatment in the literature (Harms 1973, Kiparsky 1973b, Vennemann 1974a, etc.) and yet received no satisfactory resolution, and since natural loanword phonology suggests a promising alternative analysis, we will conclude this sixth chapter with a brief examination of this phase of Hyman's Nupe proposals.

Although Nupe evidences a surface five-vowel system

(24)
i		u
e		o
	a	

Hyman proposes an underlying seven-vowel system

(25)
i		u
e		o
ɛ	a	ɔ

and a context-free rule

(26) $\begin{bmatrix} V \\ +low \end{bmatrix} \rightarrow \begin{bmatrix} +back \\ -round \end{bmatrix}$

which will account for the absolute neutralization of /ɛ/ and /ɔ/ as the low surface vowel [a].

This proposal rests in part on the existence of the General Assimilation process already mentioned which palatalizes consonants before the unrounded front vowels and labializes (and velarizes) them before back rounded ones, resulting in the following derivations:

(27) Ci $\rightarrow$ $C^{y}i$

Ce $\rightarrow$ $C^{y}e$

Cu $\rightarrow$ $C^{w}u$

Co $\rightarrow$ $C^{w}o$

But forms like

(28) /ēgwā/ "hand" /twá/ "to trim"

/ēgyà/ "blood" /tyá/ "to be mild"

/ēgā/ "stranger" /tá/ "to tell"

reveal a more complex distribution and foster a conclusion that either Nupe also contains underlying palatalized and labialized consonants (the concrete solution) or a more abstract analysis is in order for an underlying low vowel /a/.

Hyman's personal solution is that the forms in (28) have underlying forms with the sequences /go/, /gɛ/, /ga/ and /to/, /tɛ/, /ta/ respectively; that the rules of palatalization and labialization apply in the cases involving an underlying vowel other than /a/; and that these rules are followed by absolute neutralization rule (26). Such a solution is presumed to have the added advantages that it simplifies the distribution of phonemes for Nupe as well as allowing for simplification of other rules of Nupe.

It is at this point that Hyman's appeal to loan phonology becomes crucial. The neighboring language of Yoruba possesses the same seven-vowel system Hyman has projected for Nupe. Nupe is observed to borrow extensively from Yoruba, and when Yoruba words like those in (29) are borrowed Nupe speakers consistently render them as shown by the Nupe adaptations also given in (29).

(29)	YORUBA FORMS	NUPE BORROWINGS	GLOSSES
	[kɛ̀kɛ́]	[k^{y}àk^{y}á]	"bicycle"
	[ɛ̀gbɛ̀]	[ègb^{y}á]	Yoruba town name
	[tɔ̄rɛ̄]	[t^{w}ār^{y}ā]	"to give a gift"
	[kɔ́bɛ̀]	[k^{w}áb^{w}à]	"penny"

Accepting borrowing Principle I discussed above, and accepting the vowels in (25) for Nupe, Nupe speakers can be assumed to make the following analysis. Since the vowels in the Yoruba utterance [k8k8] can be recognized as acceptable underlying forms, they are lexicalized as such according to Principle I. Since the consonants of this utterance are also Nupe underlying segments, they too are lexicalized by Principle I; but both the Nupe absolute neutralization rule (26) and palatalization process (27) being automatic native rules must apply yielding the surface form with low mid vowel and palatalized consonants. The full analysis is given here as (30).

(30) POSSIBLE NUPE RENDERING OF YORUBA [kɛ̀kɛ́]

[kɛ̀kɛ́] ↑ /kɛ̀kɛ́/ ↓ k^{y}ɛ̀k^{y}ɛ́ ↓ [k^{y}àk^{y}á]

Opponents of "abstractness" in phonology have attacked Hyman's analysis of Nupe on several fronts and (excepting the loanword data which seems to provide strong independent support outside of the language system itself) these attacks have been often successful. Vennemann (1974a) demonstrates that by appealing to the two strong conditions of Natural Generative Phonology -- the No-Ordering Condition and the Strong Naturalness Condition -- a unique concrete solution can be given without appeal to extrinsically ordered rules (the Strong Naturalness Condition having ruled out in the first place the Nupe rule of absolute neutralization). Of course

Vennemann's solution remains highly suspect itself, since his own Strong Naturalness Condition appears to be an overly strong device in generative theory.

Kiparsky 1973b (also Crothers 1973) demonstrates further that Hyman has overstated the case against the concrete solution and perhaps exaggerated the degree to which it would complicate the grammar of Nupe (some Nupe rules actually turn out simpler under the concrete analysis). In fact all that seems to establish the case for Hyman's abstract analysis is the loanword argument itself, and this receives only a partially satisfying reanalysis from Kiparsky and goes altogether untreated by Hyman's other critics (e.g. Harms 1973 simply dismisses the loanword data as largely unresolvable and therefore irrelevant!). Kiparsky's treatment of the borrowings (taken up below and in Chapter Seven) offers a plausible enough explanation for why Nupe speakers are motivated to make the substitution that they do; it says nothing, however, about the method of analysis by which speakers arrive at such an analysis.

If Hyman's account of Nupe borrowing could be revealed to be subject to a reanalysis, then it would seem that the final support for an abstract vowel system and rule of absolute neutralization could be dispatched. And once a re-examination of Hyman's loanword principles is made in light of the evidence from Natural Phonology, this is exactly what occurs.

Hyman's treatment of the borrowing data is objectionable foremost in terms of the assumption he makes about the types of rules that speakers apply in the course of nativization. If speakers perceive underlying representation for loanwords to be in terms of a shallow

phonemic representation, then the argument that Nupe speakers would perceive Yoruba /ɛ/ as underlying (regardless of whether or not it appears in the Nupe lexicon) seems first to disappear. If the rule of neutralization Hyman proposes is an active process of Nupe (his own contention), it would serve to neutralize an underlying distinction and establish /a/ as underlying at the level of shallow phonemic representation (cf. Section 3.1.2). Its effect is morphophonemic and not allophonic, which leads us to the second point, which is that only allophonic processes (provided context-sensitive processes are being applied) will act upon the loanword (in early stages of nativization) once it has been perceived as an underlying form, again ruling out the analysis in (30). Also, the ordering in (30) of the allophonic process of palatalization before a clearly morphophonemic process represented in (25) would, should this analysis stand, seriously damage the version of Natural Phonology given with previous chapters.

What analysis do speakers make, then, that will account for the phonetic rendering of Yoruba [kɛ̀kɛ́] as [k^{y}àk^{y}á]? Consider the analysis in (31).

(31) ALTERNATIVE NUPE RENDERING OF YORUBA [kɛ̀kɛ́]

[kɛ̀kɛ́] ↟ kɛ̀kɛ́ ↟ /kàká/ ↟ /k^{y}àk^{y}á/ ↓ [k^{y}àk^{y}á]

Assuming that /ɛ/ is not an underlying segment of Nupe, the speaker would be faced here by a situation where he could not analyze this form as an underlying form; nor could he interpret it as a derived form in his native system. The proposal of Natural Phonology at this juncture would be that "if a segment can not be analyzed as an admissible form in the native system, it will be represented in

the lexicon as it is and finally will be restructured by the dominant [paradigmatic -- PCB] native rules" (Ohso 1971, page 4). The speaker, then, arrives at acceptable underlying vowels by applying to the Yoruba word he hears a context-free process of his grammar which has the same form as Hyman's original rule (26). But we must then account for the palatalized consonants. I will assume here that the speaker also chooses to view the Yoruba form he hears as [kɛ̀kɛ́] as one with underlying and even surface palatals. He therefore applies a second context-free process in order to arrive at the proper underlying representation /k^{y}ak^{y}á/ which then (since it demands no native allophoneic processes) surfaces as the phonetic form. (Recall that the concrete analysis assumes that Nupe does have underlying palatal consonants before /a/.)

But why should the speaker choose such an analysis, particularly when the perceived foreign word already had consonants that were perfectly acceptable as both underlying and surface segments?

Kiparsky has suggested that what is operating here is a desire by Nupe speakers, likely to be familiar with Yoruba and to some extent even bilingual, to provide the Yoruba combinations of Ca, Ce, Cɛ, Cɔ, and Co with distinct representations in Nupe. To pronounce Yoruba Cɛ as Ca would make this segment in no way distinct from the way he would, in some other borrowing, pronounce original Yoruba Ca. Kiparsky's theory of borrowing (presented in detail in Kiparsky 1973b, pages 112-13) proposes that under those conditions marked by considerable bilingualism, one target in the strategy of borrowing becomes that of preserving distinctness among the words of the lending language by utilizing the "phonetic repertoire" of the native borrowing language.

The difficulty with this approach, on the other hand, is that it makes no precise predictions as to what choices will be made from among segments of L_T to maintain the desired contrasts in forms L_S. The Natural Phonology approach to borrowings seems to remain the single empirically verifiable analysis which features just such predictions.

NOTES

[1]That there are so few useful loanword studies since the advent of the generative phonological model has mostly to do, it seems, with an excessive "abstractness" in the generative analysis. One outcome of this overblown "abstractness" pregnant with implications has been captured by Schane:

> The rules which one finds in generative descriptions are almost entirely those which I have characterized as having a morphophonemic effect, or else they are important phonetic rules which precede morphophonemic ones. Since there are so few "low-level" phonetic rules, it follows that the output of any of these generative phonologies must be a surface representation quite similar to a classical phonemic one (1971, pages 519-20).

The implications for loanword phonology are more directly stated by Lovins, who observes

> To the extent that generative phonologists have failed to distinguish rules from processes, they are thus bound to find loanwords fraught with rule exceptions (1974b, page 249, note 12).

and further

> It is no coincidence that loanwords have appeared exceptional to more and more rules of L_t as generative phonologists have since increased the depth of underlying representations. A common response to this phenomenon has unfortunately been more in the direction of "there's something strange about loanwords" than "there's something strange about the rules we're writing" (1973, page 43).

[2]The claim explicitly made here that borrowing might be done by monolinguals directly from the speakers of the source language might seem indefensible. Yet the language contact situation for MCS, discussed further in the notes to Chapter Seven, provides an excellent case for verification. A large population of monolingual and nearly monolingual speakers in Miami undergo constant exposure to numerous English words and phrases (brand names, place names, etc.) emanating from the surrounding dominant culture by means of radio and television broadcasting without achieving any significant degree of bilingualism. Of course one problem of considerable significance here as elsewhere is the difficulty of properly delimiting what constitutes full or even partial bilingualism.

[3]For a discussion of implications of loanword data in the light of historical linguistics the reader is referred to the concluding

chapter of Holden's dissertation.

[4]Working in a framework apart from Natural Phonology, Holden (1972) also observes that binary distinctive features are of marginal importance for predicting substitutions that take place in loanword phonology and that instead it is the particular phonetic manifestation of each individual segment that has importance in this respect.

[5]A more complete explanation is as follows:

> If some foreign segments cannot be analyzed as admissible underlying representations in the native system, they will be registered as violations of the rules which prohibit them. Since those rules do apply to them, no hearer would ever have the chance to notive the irregular representation in the lexicon of the borrower. He will, therefore, treat them just like the native forms in learning and represent them in the lexicon without any of the irregularity observed in the borrower's lexicon..... The children who are learning a language will finally revise the innate system of rules so that admissible representations will be achieved for all the forms they hear from adults. But it seems to be very difficult for adults who have once acquired a phonological system to revise it to comprise new segments or new sequences of segments. In many cases perception itself seems to be constrained by the native system, so that speakers perceive foreign sounds in terms of the native phonological system. Thus when some segments have to be registered in violation of native rules, the borrower will change his inadmissible underlying representations to admissible ones sooner or later, unless he learns to revise the native system to allow them. In other words, the inadmissible forms which were first registered in the lexicon will be changed to admissible ones by the application of the dominant rules which constrain them -- a process I refer to as the "restructuring of underlying representations" (Ohso 1971, page 4).

CHAPTER SEVEN

LOANWORD PHONOLOGY IN THE MIAMI CUBAN DIALECT

> What will emerge from the collision of two sound systems is by no means wholly predictable, any more than is any other linguistic phenomenon; simply finding a plausible explanation for the kinds of things that happen, in relation to other things we know about the languages involved, may be something of a challenge.
>
> -- Julie Beth Lovins

7.1 Extent and Types of Borrowing

I base this chapter on two assumptions about linguistic borrowing which parallel those found in Holden's somewhat superficial though generally insightful dissertation (Holden 1972) on the integration of foreign loanwords into native phonological systems. These are the assumptions that: (1) the type of borrowing phenomenon of interest to and ultimately available to any preliminary phonological study is a simplified kind of linguistic borrowing whereby monolinguals or near monolinguals adopt foreign words rendered in their own language by the bilinguals of the same linguistic community, and (2) we have approximate access to the source phonetic forms of borrowings as they sound in the original donor language and are able to make reasonably accurate assumptions about when the borrowed forms have been fully assimilated phonologically into the target language system.

Furthermore, it will be necessary that we largely ignore here such eventually inescapable questions as:

(1) What are the changes in borrowings brought about by orthographic assimilations?

(2) What sociological factors shape and influence the direction of linguistic borrowing?

and

(3) What is the role played by morphological, semantic, and other non-phonetic factors in the assimilation of borrowings?

In short, limitations of space as well as our present imprecise understanding of borrowing phenomena work to require that we overlook many of the complex and unresolved issues involved in the complete study of loanword phonology. For a statement and analysis of some of these more perplexing problems the reader is referred to Chapter Two of Holden's useful dissertation. More positively, within the range of this current study, what this present chapter does hope to accomplish is, first, to examine the role of innate phonological processes as a central feature in initial stages of phonological assimilation of loanwords; and, second, to illustrate the complete degree to which Stampe's system of Natural Phonology resolves the most pressing questions about the nature of phonological assimilation processes in the large scale adaptation of nativized loanwords.

The notion that productive natural processes rather than abstract and exceptional rules are fundamental to the processes of borrowing is neither entirely new nor the exclusive property of Stampe and the natural phonologists.[1] Johns (1972, page 551), for instance, remarks that "it would seem that a language's ability to borrow words is at least largely determined by the shapes permissible at the surface level, especially in the realm of proper names, where little combinability is required." This is compatible with assumptions by Clayton, Shibatani, and others (see Section 4.1.2) that surface patterns and not underlying restrictions

are exclusively productive in phonology. Discussion above in Chapter Four illsutrates, however, that Stampe's version of phonology provides a more reasonable hypothesis than heretofore about what the actual nature of such surface patterns is: i.e. active phonological processes rather than static conditions or psychologically untenable and formally amorphous restrictions on segments and rules.

This seventh chapter examines specifically phonological phenomena in loanwords from Miami Cuban Spanish (here MCS). The corpus under study will be several dozen selected English borrowings recovered from tapes of interviews with twelve primary native informants (see Appendix Part One), as well as from supplemental tapes recorded from Miami Spanish-language radio programming.[2] While no single chapter could hope to encompass a complete study of the numerous problems which surround loanword phonology in this single dialect, the following discussion should at least serve as one sound rationale for tentatively selecting the Natural Phonology approach to loanwords over all other earlier methodologies.

I begin this chapter with a brief discussion of the types of foreign loanwords this work will examine, as well as types which will not be considered relevant to a treatise on the phonological processes involved in borrowing. An Appendix (Part Three) to this study provides approximately one hundred additional borrowings which are illustrative of specific loanword phenomena discussed more explicitly in these pages. Sections 7.2 and 7.3 explore in finer detail applications of syntagmatic and paradigmatic natural processes, these being together the two primary mechanisms ("backwards applications of lower level processes and forwards applications of higher level ones") by which phonetic borrowings are first lexicalized by native speakers of the target system. It will be shown that the no-

tion of a "closest segment" between two languages in contact -- one of the fundamental concepts of all earlier approaches to linguistic borrowing -- is largely clarified and explicated once the role of the syntagmatic and paradigmatic processes is fully exposed. A number of theoretical issues touched upon in earlier sections of this dissertation will now also draw further discussion in Section 7.2 and Section 7.3 in light of the loanword data from Miami Spanish. A few of these issues will be those involving: (1) empirical evidence for Stampe's notion of a shallow level of underlying phonological representation characterized in the literature on Natural Phonology as the natural phonemic level; (2) evidence for the reality outside of child language of dominant paradigmatic processes which function as actual substitutions filtering the lexical structures; (3) the viability of Kiparsky's (1973b) recent theory of borrowing which distinguishes "casual contact" situations from borrowing under the conditions of "extensive bilingualism" where simply maintaining distinctness between lexical items in the lending language becomes the overriding goal; (4) the justification for proposed phonemic restructuring (see Hammond 1975, 1976) in the underlying inventory of segments which is based almost exclusively on the observed surface patterns in the Miami dialect of Cuban Spanish.

Also drawing further discussion and support in light of data to be surveyed with this chapter will be Lovins's (1974a, pages 16-7) valuable observation that loanwords which must be noted as exceptions to the set of native phonological "rules" remain exceptions to the phonological "rules" and morphophonemic processes alone, yet never violate the true allophonic processes which comprise the only actual constraints on pronounceability in a native phonological system.

For the clarification of loanword types, Einer Haugen's definitive article (Haugen 1950) on the structural approach to loanword phonology still provides a most useful point of reference. Haugen's definitions of distinct types affords an excellent place to begin. In providing the following descriptive terminology, however, Haugen warns that "borrowing as here defined is strictly a process and not a state, yet most of the terms used in discussing it are ordinarily descriptive of its results rather than of the process itself" (1950, page 212). With such a caution held firmly in mind, we may proceed to the following definitions of loanword types.

> Loanword is the vaguest of the group, since it may include practically any of the others. But it is ordinarily limited to such terms as AmE shivaree "an uninvited serenade of newlyweds" from Fr. charivari, in which speakers have imported not only the meaning of the form but also its phonemic shape, though with more or less complete substitution of native phonemes (1950, page 213, with my italics -- PCB).

> Hybrid is sometimes used to distinguish loanwords in which only a part of the phonemic shape of the word has been imported, while a native portion has been substituted for the rest. Thus PaG has adopted AmE plum pie as [blaUmәpaI], in which the morpheme [paI] has been imported, but the native [blaUmә] has been substituted for plum. In this case the borrowing speakers must have analyzed the compound into its component morphemes while they were borrowing it, or else they could not have made this partial substitution. This distinction puts the process on a different level from the merely phonemic substitution of the preceding type ["loanword" -- PCB], so that we are required by the evidence to postulate a morphemic substitution which operates independently of the phonemic (page 214, with my italics -- PCB).

> If we turn now to loan translation (known in French as a calque), we encounter such examples as the French presqu'ile, German Halbinsel, modeled on Latin paeninsula; or German Wolkenkratzer, Fr. gratte-ciel, Sp. rascacielos, modeled on E skyscraper..... Instead of substituting only one half of the word, the borrowers here have analyzed and substituted both halves. They have imported a particular structural pattern, viz. the combination of the two constituents into a compound expression with a new meaning of its own not derivable by a simple addition of the two parts (page 214, my italics -- PCB).

> Loanwords show morphemic importation without substitution. Any morphemic importation can be further classified according to the degree of its phonemic substitution: none, partial, or complete... ...Loanblends show morphemic substitution as well as importation. All substitution involves a certain degree of analysis by the speaker of the model he is imitating.....Loanshifts show morphemic substitution without importation. These include what are usually called "loan translations" and "semantic loans"; the term "shift" is suggested because they appear in the borrowing language only as functional shifts of native morphemes (1950, page 215).

That the extensive corpus of English borrowings adapted by speakers of MCS illustrates the full range of Haugen's borrowing types has already been amply illustrated in at least one study. In an unpublished thesis treating the lexical and syntactic impact of English on MCS, Roberto Fernández (1973) formally adopts a modified version of Haugen's original definitions and resulting classifications into loanword types.[3] These formal categories and some representative sample from each anticipate the full loanword corpus (Appendix Part Three) utilized for this current study and drawn in part from Fernández's original word lists. I provide next in (4) through (7) only a general sampling of the various types.

(4) LOANSHIFTS, which are especially prevelant in this dialect, occur when a word already extant in the target language acquires by means of foreign influence a new meaning which is modelled on some semantically or phonetically similar form of the source language. Haugen has earlier defined these as borrowings where there is semantic substitution of meanings within the native system without any importation of foreign morphemes or foreign phonemes into the native language system. Some examples of loanshifts reported by Fernández to have a high frequency of usage in MCS may be provided from his list of approximately eighty such forms.

LOANSHIFT	CURRENT MEANING IN MCS	ORIGINAL SPANISH
actualmente	as a matter of fact	at the present

alarma	alarm clock	signal of danger
apología	expression of regret	defense or praise
atender	to be present at	to pay attention to
carácter	eccentric person	person's essential nature
competición	relationship existing between those struggling to attain something	actual struggle to obtain something
espíritu	cheerfulness, vivacity	trend noticeable in the spiritual manifestations of a human group
lectura	academic presentation or severe scolding	act of reading
pipa	long tube to convey water, gas, etc.	pipe for smoking
relativo	family relation	what is opposed to things absoluto

(5) LOAN TRANSLATIONS feature a phrase from the source language which has been translated literally and therefore incorrectly into the target native language. These represent cases where speakers have "imported a particular structural pattern, viz. the combination of the two constituents into a compound expression with a new meaning of its own not derivable by a simple addition of the two parts" (Haugen 1950, page 214). Among examples from MCS (Fernández 1973, Chapter Five) we find such typical combinations as the following few:

LOAN TRANSLATION	GLOSS	STANDARD SPANISH
cortar clases	to cut classes	no asistir
fuera de línea	out of line	salido del tiesto
jugar la parte	play a role	hacer el papel
no hay cartero	there is no mail	no hay correo
nombrar después de mí	to name after me	poner el nombre

(6) HYBRID CREATIONS are the first borrowing type of any phonological interest. Here a root morpheme from a source language is combined with a derivational affix morpheme in the native language. The result may be

viewed as one division within Haugen's larger category of loanblends: "In reproducing the forms of another language speakers will frequently go farther in their adaptation than merely to substitute native sounds and inflections for foreign ones.....They may actually slip in part or all of a native morpheme for some part of the foreign, as in AmPort. alvachus 'overalls', where the native prefix al- has been substituted for the E o-" (1950, page 218). As Haugen proceeds to note, however, such morphemic substitutions are only readily observable when the phonetic results differ from those that would be derived from any straightforward phonological substitutions.

Among Fernández's examples of hybrid creations are the numerous new verbs which have been formed from an adapted (and phonologically nativized) English verb form plus the Spanish first conjugation verbal suffix -ar (which is evidenced by these samples to be clearly the productive verb-forming conjugation suffix in Spanish).[4] The following list of lexical items is drawn in large part from Fernández, though the recorded pronunciations are those taken from my own taping of Miami informants. Since the non-Spanish element in each case here is the verbal stem, these items along with those of the subsequent category (hybrid compounds) provide a number of revealing phonological adaptations in the process of nativization.

NEW SPANISH VERBS		RELATED ENGLISH VERBS
achiquenearse	[ačikeneárse]	to chicken out
afordear	[afordeár]	to afford
aprochar	[aproč̌ár]	to approach
bosear	[boseár]	to boss (around)
bostear	[bohteár] [boteár]	to bust (to arrest)
bribar	[briƀár]	to bribe
buquear	[bukeár]	to book (into a hotel)

cachear	[kačeár]/[kačár]	to cash, to catch
chitear	[čiteár]	to cheat
chopear	[čopeár]	to shop
cuitear	[kwiteár]	to quit
deitear	[deiteár]	to date
dropear	[dropeár]	to drop (a class)
esquipear	[ehkipeár]	to skip (a class)
espelear	[ehpeleár]	to spell
failear	[faileár]	to file
feiquear	[feikeár]	to fake
flachear	[flačeár]/[flačár]	to flash (in radio commercials with expressions like "...in front of the flashing green lights")
flonquear	[floŋkeár	to flunk (a class)
frisear	[friseár]/[frisár]	to freeze
guaxear (waxear)	[guakseár]	to wax (a car)
janguear	[haŋgeár]	to hang (clothes)
lonchear	[loŋčeár]/[loŋčár]	to (eat) lunch
liquear	[likeár]	to leak
meilear	[meileár]	to mail
mixear	[mikseár]	to mix
pompear	[pompeár]	to pump
postear	[pohteár]	to post (grades at school)
rilaxear	[rilakseár]	to relax
sainear	[saineár]	to sign

(The transcriptions here being legitimate approximations of the less-frequently-used infinitive forms -- in actual usage the varied pronunciations of such infinitives alternate rather freely between [eár], [iár], and [ár] for the infinitive suffix)

Additional types of hybrid creations are also possible, displaying applications of a wider range of native Spanish word-formation (morpho-lexical) rules:

biliadora	[biliadóra]	derived from English verb "to bill" plus native Spanish noun suffix -or(a)

chiteador	[čiteaɣor]	derived from English verb "to cheat" plus Spanish derivational suffix -ador
culísimo	[kulísimo]	derived from English adjective "cool" plus Spanish intensifier form -ísimo
sagüesera	[saɣweséra]	derived from the English noun "southwest" plus Spanish derivational suffix -era

(7) HYBRID COMPOUNDS provide a second type variation on Haugen's original notion of loanblends. In this case we find phrases in the target language created by combining native items with phonologically nativized words from the source language. Again the adopted word in expected fashion displays normal processes of nativization within the target system.

HYBRID COMPOUNDS		GLOSS
algo foni	[fóni]	something funny
armar troba	[tróβa]	to stir up trouble
Bankamigos	[bãŋkamígoh] [bãŋkamígo]	The Friends Bank
broder mío	[bróðer]	my brother
coger un incomplí	[ĩŋkomplí] [ĩkomplí]	to take an incomplete (not finish all word for a college course)
coger un tan	[ũŋ tãŋ]	to get a sun tan
día bisi	[bísi]	busy day
estar avéilabol	[aβéilaβol]	to be available
estar diprés	[dipréh]	to be depressed
estar erli	[érli]	to be early
estar lei	[léi]	to be late
estar ronin	[rõníŋ]	to be running (for office)
estar en probeichon	[proβéičoŋ]	to be on probation
estar singuel	[síŋgel]	to be single
hacer espí	[ehpí]	to speed up
ir de chopin	[čópiŋ]	to go shopping
por fon	[fõŋ]	for fun
¡qué cul!	[kúl]	how cool (neat)!
¡qué nais!	[náih]	how nice!

¡qué sui!	[swí]	how sweet
sentirse daun	[daũŋ]	to feel down (depressed)
sentirse op	[óp]	to feel up (in good spirits)
ser chai	[čái]	to be shy
tomar un brei	[bréi]	to take a (coffee) break
tener fon	[fõŋ]	to have fun
tener pul	[púl]	to have pull (influence)

The MCS loanwords we have listed in (4) through (7) make up a large portion of the borrowings encountered in this dialect, and we will return presently to an account of a number of individual forms included here under (6) and (7) which display features of considerable interest to a phonological study of borrowing. First, however, it is necessary to observe that the classifications of loanwords presented so far in this chapter do not yet allow for a frequent and perhaps even predominant type of borrowed lexical item. This is the word which retains something of its original form in the source language while at the same time undergoing all "necessary" adaptations to the phonetics of the borrowing system. English elements of the hybrid creations (6) and hybrid components (7) have undergone an adaptation of this type with most speakers, yet there are numerous other examples which display no such mergers with any native Spanish morphemes. For example, there are the frequently encountered borrowings in (8) which also demand further explication in subsequent sections.

(8) ISOLATED LEXICAL BORROWINGS

[haihkúl]	high school
[kaskár]	cash card
[dãũntãũ]	downtown
[sáŋwiš]	sandwich
[sáŋwiše]	sandwiches
[čów]	show

[panãn]	Pan Am
[panán]	
[panám]	spelling pronunciation!
[ékspor]	export
[sausés]	southwest
[sauséh]	
[norsés]	northwest
[norséh]	

An alternative classification of loanword types, acommodating forms like those found in (8), might be one suggested by Theodore Beardsley (1973) in his preliminary analysis of English influences in the Spanish dialect of Key West. Beardsley's classification (which is based loosely on similar classifications in Espinosa 1957, Murphy 1954, Ortiz 1949, and elsewhere) in its shortened form captures four distinct (and in two cases parallel) loanword types:

(9) Words which in borrowing maintain their original forms (i.e. presumably "phonemic" form) and their original phonetic shapes once adopted into the target system. Beardsley gives an example of the Key West Spanish form la sueda [swéđa] from English "sweater", explaining that the Southern dialect of English which provides this form has the normal pronunciation [swɛ́Də]. Here it might be assumed in light of principles of borrowing discussed below that paradigmatic processes eliminating segments which are not acceptable at either the abstract or surface level are applied by the Spanish speaker in order to achieve the phonetic form /sweda/ (viz. /ɛ/ becomes /e/, /D/ becomes /d/, and /ə/ becomes /a/). Since this underlying form would also be an acceptable surface form, no additional allophonic processes need now be applied in achieving the phonetic form (with a possible obvious exception of /d/ → [đ] between vowels).[5]

(10) Words which maintain their original phonemic form but are adapted to the phonetic system of the borrowing language. Here Beardsley

cites the example of the Spanish form el suéter. It must be assumed in this case that Spanish borrowers (perhaps influenced by spelling) assume a phonemic rendering /sweter/ for the form and then ignore the exclusively English allophonic process which converts underlying /t/ in this environment to [D].

(11) Direct translation of foreign words or phrases into the native language system. An example here would be rueda de manejar for the English steering wheel.

(12) False cognates, in the cases of which a phonetic similarity to some word in the native system results in assigning an incorrect meaning to the original foreign lexical item. For example, Spanish gabinete might be adopted as equivalent to the English cabinet.

Within this second approach to classification we will immediately recognize that (11) direct translations and (12) false cognates correspond comfortably to categories (5) loan translations and (4) loanshifts from the original Haugen/Fernández classification introduced earlier. Yet Beardsley's distinction between types (9) and (10) allows a useful and thoroughly motivated division which is not made explicit within the categories as first formulated by Haugen.

At this point it may prove enlightening for further discussion to attempt some combination of these two proposed catalogues of borrowings in order to achieve a more reasonable system of classifications. This revised classification would hopefully place greater emphasis on an essential typological division featured in Holden's (1972) general discussion of borrowings. This division would be between (a) the adaptation of foreign phonological segments to the phonetic requirements of the target system, and (b) the adoption of new and previously inadmissible

segments by the phonology of the target system.

First there is the category we will label TYPE ONE of those forms borrowed from the source language which are made up of segments in all cases identical to those of the target system and therefore readily adopted without any further phonetic alteration. This would correspond to Beardsley's first category (9) and would be a type accounting for only a small (perhaps even minute) percentage of borrowings across all language contact situations. This subdivision might be further characterized as illustrating the case of phonological borrowing without the element of phonetic interference (this latter element, of course, usually being taken as the only true subject of loanword study).

Secondly, we would have the class of borrowings of TYPE TWO which introduce new and unprecedented segments or sequences of segments into the target language system. At times loanword adaptation to the features of the target system is incomplete (accounting for the notion of the stratified lexicon of formatives or segments marked [+native] and [-native]) and some features of borrowings remain which do not match with this native system. This type of loanword phenomenon and the multiple theoretical approaches which have arisen to account for it are systematically reviewed in the fourth chapter of Holden 1972. Another study which explores the adoption of foreign elements within the segmental phonology of several American Spanish dialects, with particular attention given to the substratum theory of foreign phonological influence, is Cassano 1970. And among the limited examples of apparent English influence within the native phonology of MCS there are scanty examples which are perhaps best dismissed from consideration for reasons provided in Section 7.2. I cite only several examples:

(13) Adoption of an English voiced palatal affricate [ǰ] in a few frequently repeated names, e.g.

[ǰáksombil]/[ǰáksõŋbil] for "Jacksonville"

[ǰõŋsõŋ] "Johnson"

[ǰíɟiš] "Yiddish"

but compare

[nuyérsi] "New Jersey"

[yeralfór] "Gerald Ford"

(14) An apparent adoption of English retroflex [ṛ], with the additional consequence of rendering most difficult proper perception and unquestioned identification (by the linguist) of the preceding reduced vowel in forms like

[fUṛféɟeral] in "First Federal Bank"[6]

(15) Occurrence through English influence of a standard Spanish phoneme in an environment where that segment is normally not permitted by native morpheme structure or word structure requirements, e.g.

[mayámi bíč]/[mayámi bíš] "Miami Beach"

[dáɸalyu tí gránt] "W. T. Grant"

[wúlwur?] "Woolworth's"

Finally, there is a third more extensive class of borrowed forms, TYPE THREE, which are fully (or at least largely) adapted to the morphological system and phonological system of the native target language. A first subdivision within this final category would be that in which there is morphological adaptation. This would include what Haugen and Fernández together label as hybrid creations, but the category would likely encompass more than just those adopted foreign forms which have appended to them highly productive derivational affixes from the

native system. For instance, more general native word-formation rules of the type discussed briefly in Section 4.3 might be expected to play some role here.[7] However, since we are concerned with phonological and not morphological regularization in this chapter, we will not further pursue this interesting question at this present juncture.

A second category within loanwords of TYPE THREE would be presumably the largest and what phonologists take to be the only truly interesting subdivision: adopted foreign lexical items in which there is phonological adaptation of the foreign phonetic elements to the structural demands of the native target-language system. Of course those forms which undergo morphological adaptation may at the same time be subject to processes of phonological and phonetic adaptation as well. Observe, for example, the MCS form [wikénes] ("weekends") which undergoes both a process of final cluster reduction and the word-formation rule which governs Spanish plurals.[8] Such typologically complex forms are rare, however, and the bulk of examples in TYPE THREE would correspond rather more precisely to the second category of borrowings -- here (10) above -- defined by Beardsley as more straightforward phonetic adaptations. It is this category that will be exclusively featured in the remaining two sections of this chapter to follow.

7.2 Phonological Nativization and Phonological Adoption

Returning to concrete examples of English phonological borrowings in MCS, at least the following initial observations would all seem to be prerequisite to any very perceptive studies. To begin with, the large collection of forms labelled by Fernández as loanshifts and loan translations, though they are substantial (together they account for 116 of 268 total borrowings mentioned in Fernández 1973), remain of

no interest whatsoever to this present strictly phonological investigation. These involve exclusively words from the native component of the lexicon and the only adjustments made by a native speaker are positively semantic in scope. In fact, it is ultimately semantic and not lexical elements at all which are in these instances being borrowed.

Among the more legitimate cases of genuine lexical borrowings (the takeover and utilization of actual foreign words and not just adoption of new and revised connotations for existing native words drawn from parallels with foreign elements), those which would fit into TYPE ONE of our presently revised classification, where segments and combinations of segments are identical to those already found in the native component and therefore no phonetic adjustments are demanded, are also of relatively little interest to a study bent on justification of a natural phonologist's approaches to generative grammar. Borrowings of this type are considerable but do not seem to be predominant in MCS, being not as numerous as actively nativized forms of TYPE THREE. Some examples:

(16) TYPE ONE PHONETICALLY-ACCEPTABLE BORROWINGS

[hái] in "high school"

[mayámi] "Miami"

[miámi] "Miami"

[hayalía] "Hialeah" though heard also as [xayalía]

[níkson] "Nixon" though sometimes [níksoŋ]

Borrowings of TYPE TWO, where new segments are introduced into the target system, are rare among the loanword phenomena surveyed from MCS and must therefore also remain outside the primary concerns of this present chapter. We must leave aside therefore the important theoretical question of whether or not such adopted features are to be treated as

unique yet fully acceptable elements in the native system which has now been modified to accommodate them, or whether they must continue to be distinguished for their "foreignness" in some formal way (cf. Holden 1972, pages 148 and following). A few illustrations of borrowings of this type are provided with (13) through (15) and their impact on the native system is the subject of superficial discussion in Section 7.1. With the overwhelming majority of loanwords recorded in this study, however, an observation long prevelant among Spanish linguists seems to be nominally verified: although lexical borrowings in this and other dialects of Spanish are quite numerous, speakers continually treat these newly acquired words as though they were fully native Spanish words, reflecting only the segments and processes of the native phonological component. In this sense this chapter also verifies one major proposal of Natural Phonology: the phonological assimilation of loanwords provides persistent evidence for both active constraints (i.e. processes) shaping underlying segmental inventories as well as sequential phonetic constraints in the target language. In exploring evidence for these loanword claims of natural phonologists, such as those sketched above in terms of formal hypotheses in Chapter One (pages 48 and 49), it also becomes abundantly apparent, however, that certain inherent flaws remain unrectified in Stampe's pioneer model. At the same time, certain proposals for significant and largely successful revision in the theory also readily suggest themselves.

7.3 Conclusions and Revisions in Natural Loanword Phonology

Formal hypotheses constituting a principled theory of borrowings may be culled from the pioneer work of Hyman (Nupe) and Lovins and Ohso (Japanese) and collapsed as the following loanword claims:

(17) Foreign sequences of sounds are perceived by a speaker of the borrowing language or dialect wherever possible as being native underlying forms, and this involves the perception of underlying representations as being no more abstract than what Stampe calls a natural or a shallow "phonemic" level (i.e. foreign phonetic sequences which are acceptable native phonemic sequences are put in the speaker's lexicon in their foreign phonetic shape, which is perceived as a native phonemic shape).

(18) When a loanword is perceived by the speaker as equivalent to a contextually-derived native sequence, then the form will be lexicalized it its presumed phonemic form and subjected to the recognized native processes which will derive the appropriate surface form.

(19) If the perceived loanword can be interpreted as neither an acceptable phonemic form nor a permissible native derived sequence, the speaker will be left with the strategy of applying any available paradigmatic or syntagmatic processes existing in his native grammar in order to achieve an acceptable lexical representation.[9]

Corollary to these borrowing principles are quite obviously a number of fundamental assumptions about the nature of abstract representation and the notion of phonological processes active in the native grammar. Among the most vital are the following set.

(20) Speakers hold some psychologically real notion of a shallow phonological representation for strings that can be denoted as "phonemic" form, and a lexical representation corresponds to such shallow representations in all but forms demonstrating complex morphophonemic neutralizations.

(21) Loanwords in the general case are sensitive only to allophonic

or nondistinctive type alternations and therefore do not require deeper morphophonemic ("systematic phonemic") representations (such as those assumed for all underlying lexical representations in the standard model). Lovins (1974b, page 243) observes that it is this latter fact about loanwords which accounts for frequent assumptions about their "exceptionality" among traditional generative phonologists.

(22) Morphophonemic processes must be assumed, as is the rule in Natural Phonology generally, to be deeper than allophonic processes and thus rendered unavailable for normal cases of loanword analysis simply through certain natural ordering relationships within the grammar.

From the above list of loanword assumptions, (17) and (19) especially would seem to account for one recent analysis of a borrowing relationship which seems at first glance to represent a contradiction of a number of the principles deriving from the Natural Phonology account of such phenomena.

Consider the case of a constraint on Korean loanwords proposed by Masayoshi Shibatani (1973, page 98). Shibatani notes that Korean has both an apparent MSC and parallel SPC prohibiting word-initial liquids at both the lexical and phonetic levels. Since no native Korean morpheme traditionally had an initial liquid, there was no apparent (at least not manifestly) process alternating /l/ and /n/; but upon the appearance of Chinese loanwords in the language precisely such an alternation surfaces.

(23) Chinese lok becomes Korean nok "green"

Chinese lam becomes Korean nam "blue"

Shibatani in addition makes much of the fact that such loans seem to be sensitive only to a SPC and not any MSC, since independent alterna-

tions reveal the lexical status (after these borrowings have entered the language) of the phonetically-banned /l/:

(24) /lo + in/ → [noin] "old man"

/č̆o + lo/ → [č̆olo] "premature decay"

/lak + wɔn/ → [nakwɔn] "paradise"

/k'wɛ + lak/ → [k'wɛlak] "enjoyment"

If we are to assume that speakers lexicalize loanwords at a "systematic phonemic" or more abstract lexical level of underlying representation (as in the standard model), then it would appear that a Chinese loan like lok would be taken as the appropriate native underlying form, it would be lexicalized accordingly with /l/, and then it would be subjected to the native morphophonemic process neutralizing /l/ and /n/ in initial position. Yet with this analysis, the application of such a morphophonemic process would seem to nullify at least assumption (21) above, that allophonic processes and not morphophonemic processes are the ones operative in loanword nativizations. But again the claim of Natural Phonology (19) is that loanwords are taken to be perceived in terms of "natural phonemic" and not "systematic phonemic" representations. It is therefore principle (19) which seems to account best for the analysis being made here: since a form with initial /l/ is acceptable as neither an underlying (shallow phonemic) not a phonetic sequence, the speaker must rely on his innate knowledge of whatever processes within his grammar (paradigmatic, morphophonemic, or allophonic) might account for such an obviously derived form. Since he has available a highly productive morphophonemic process neutralizing liquids and nasals morpheme-initially, it would seem apparent that he recognize /nok/ as the only possible "phonemic" analysis here and lexicalize this form accordingly.

No actual "application" of any morphophonemic process would therefore appear to be involved, only the knowledge that any segment that is specified [+consonantal] and [+sonorant] morpheme-initially is at the phonemic level [+nasal] as well. It must be qualified immediately here that I am not contradicting in any consequential sense Stampe's strong contention that all such constraints are the result of active processes in the grammar rather than static conditions on morpheme structure; instead I am suggesting that it may be the speaker's awareness of the reality of such processes (just as German speakers are aware that morphemes where final devoicing has failed to apply are simply not German) and not always his actual sequential applications of such processes which is involved in his assumptions about what constitutes a "phonemic" form for individual morphemes.

If phonological borrowing can be demonstrated to operate in anything like the ways captured in assumptions (17) through (22), not only will the earlier attempts at coherent borrowing theories undergo radical revision. But also, Stampe's several speculations about the reality of the phonemic level of linguistic generalization, and his claims for the organization and productive role of innate processes which are distinct yet teleologically related types in the adult grammar, will seem largely verified. On this hopeful assumption, then, let us now return to a number of specific cases of loanword data in MCS.

Appendix Part Three provides an approximate inventory of frequently encountered MCS borrowings, discovered through my own fieldwork and arranged in terms of the loanword categories we have outlined with some precision above. This preliminary inventory makes it altogether apparent that TYPE THREE borrowings (phonologically nativized forms) predominate.

Observe the following forms for purposes of subsequent discussion:

(25) MCS TYPE THREE LOANWORDS

ENGLISH FORM	GLOSS	MCS FORM	PHONEMIC FORM	MCS ALLOPHONIC PROCESSES
[t^{h}áen]	"tan"	[táŋ]	/tán/	Velar Nasal Intrusion (VNI)
[ríyzən]	"reason"	[rísθŋ][10]	/ríson/	Vowel Reduction (VR), VNI
[nʌ́θIŋ]	"nothing"	[nósiŋ]	/nósin/	VNI
[hárləm]	"Harlem"	[hárleŋ]	/hárlen/	VNI
		[hárlE]	/hárlen/	VNI, Vowel Nasalization (VN), Nasal Consonant Deletion (NCD), Denasalization (DENAS), VR
[haémbərgər]	"hamburger"	[hambéɣE]	/hambége/	Spirantization (Spr), VR
[púlowvər]	"pullover"	[pulóβUr]	/pulóbur/	Spr, VR
[dískawnt]	"discount"	[dihkáu͡n]	/diskaun/	S-Aspiration (Asp), VNI, VN, NCD, DENAS
[Iŋkʌmplíyt]	"incomplete"	[iŋkomplí]	/inkomplí/	Nasal Assimilation (NA)
[sawθwɛ́st]	"southwest"	[sau͡wéh]	/sauwés/	S-Deletion (SD), Asp
[hówmstɛd]	"Homestead"	[hámstE][11]	/hámsted/	D-Deletion (DD), VR

The evidence of TYPE THREE borrowings presented here, sustained by the numerous parallel forms from Appendix Part Three, seems persuasive that in the normal case of loanword analysis a speaker approaches foreign sound sequences through approximating some appropriate underlying phonemic representation and then applying allophonic-type processes needed to achieve a recognizable native phonetic form. Returning to a notation utilized in Chapter Six, a single sample derivation might appear in

somewhat the following sequence:

(26) MCS TREATMENT OF ENGLISH DISCOUNT

[dɪ́skawnt] ↤ /dɪ́skaunt/ ↤ /diskáun/ ↤ /dihkáun/ ↤ /dihkáũn/ ↤ [dihkáũ]
(phonemic)

VR CR ASP VNI ND
STRESS

where, VR = Vowel Raising
CR = Cluster Reduction
ASP = S-Aspiration
VNI = Velar Nasal Intrusion
ND = Nasal Consonant Deletion

It is evident that the speaker in achieving such analyses must apply an extensive inventory of context-free vowel neutralization processes in reducing the complex phonetic vowels of a system like English to the restricted series of his own system in Spanish. These context-free vowel neutralization processes are of the type well characterized in the studies of Donegan Miller (especially 1972b) -- inter alia Neutral Vowel Lowering, Round-Vowel Depalatalization, Palatal Vowel Unrounding, Low-Vowel Unrounding, Raising, etc. -- and they comprise the identical collection of processes utilized by the infant in initially acquiring his own sparse Spanish inventory of vowels.

Other paradigmatic processes are applicable as well in determining phonemic form, e.g. that converting all cases of θ to s at the underlying level in MCS in the above loanword examples. Numerous processes applied to the perceived foreign phonetic form in such a "backwards" derivation are of the context-sensitive variety, however, as Lovins has earlier observed: "When we speak we apply allophonic processes 'forwards' to produce contextual variants; when we listen to someone else, we apply them 'backwards' to relate the allophones to their associated phonemes, automatically considering a sequence of as many segments at a time as

are involved (as context or derivates) in a derivation" (1974b, page 244). Yet even more useful here is the observation that "Likewise, in listening to unfamiliar foreign sounds, we try to relate what we hear to possible surface forms in our own language [and] these surface forms may already be acceptable underlying representations, or related to such by backwards-derivation of an allophonic process" (page 244). These processes (paradigmatic and syntagmatic) applicable in such backwards derivations of loanwords are all strictly allophonic in the sense that they recognize and eliminate contextual variants (replacing them with proper native phonemes) rather than undoing neutralizations of existing phonemes (see e.g. the discussion of the Korean loanword problem above).

Evidence provided by TYPE THREE loanwords also seems to permit a number of further pronouncements concerning the accuracy of a Natural Phonology model as envisioned by Stampe and outlined throughout preceding chapters. For example, one issue which seems to dissipate is that involving the psychological validity of an assumed "phonemic" level of analysis. Wojcik, for instance, though a proponent of Natural Phonology from the first, announces that he "has yet to understand the value of maintaining an autonomous level of phonemic representation for all words in the language" (1975, page 5); yet the point seems to be here not that linguists must retain such a level as a means of convenient analysis, but rather that speakers themselves do indeed just exactly this. The Natural Phonology treatment of loanword phonology maintains that in the general case speakers not only perceive loanwords in initial stages of borrowing in terms of shallow phonemic underlying forms but also apply only allophonic processes (those which act upon

phonemic representation) in the nativization of such phonological exceptions to their native grammatical systems.

Yet further problems involving Stampe's original notion of phonemic and allophonic remain and I propose at this point to take up two engaging examples. The first is from outside the realm of loanword phonology per se, though it most crucially affects the various related loanword claims introduced. The second derives more directly from the evidence arising from loanwords imbedded in MCS.

Consider initially the following possible rapid speech derivation of the English word question, which is brought about through a series of allophonic and morphophonemic processes applying in a strictly feeding order.

(27) RAPID SPEECH DERIVATION OF ENGLISH QUESTION

UNDERLYING FORM	kwɛstyən	
T-PALATALIZATION	kwɛst'yən	(allophonic)
GLIDE DELETION	kwɛst'ən	(morphophonemic?)
T-AFFRICATION	kwɛsc̆ən	(morphophonemic)
S-PALATALIZATION	kwɛs̆c̆ən	(morphophonemic)

The derivation represented by (27) has been called to my attention by Gary Miller (personal communication) and is admittedly consistent with my own rapid speech outputs. What is troublesome to the theories being espoused here is that such a derivation displays the reverse ordering of processes from that which Stampe's theory explicitly claims is the only ordering possible (morphophonemic/allophonic). One conclusion to be drawn in the light of (27) might again be that after all the proposal of a natural phonemic level is altogether impossible, being drawn as it is on the notion of a strict ordering between

morphophonemic processes (establishing the phonemic representation) and allophonic processes (providing non-distinctive alternation of phonemic segments). Recall that we discussed at length above in Chapter Two a similar derivation in Russian resulting in [tavaɾiž̬ bɨ] "comrade" (emp.), where an apparent morphophonemic process (voicing to ž̬) followed a clear allophonic process (voicing to ž̬).

Assuming for the sake of argument that such processes as Russian Voicing Assimilation do apply sequentially to forms like /tavaɾiš̬š̬ bɨ/ (and not simply simultaneously, thus ruling out any question of ordering), it was tentatively offered in Chapter Two that Stampe's proposals might have to be loosened to some degree to allow for the assumption that, when in the course of applying allophonic processes new environments are established where morphophonemic processes are applicable, the latter will always reapply. It is the very nature of all processes (Chapter Two) that unless suspended they are always evidenced wherever environments dictate.

The above derivation of question would also in similar fashion seem to dictate certain modifications of Stampe's schema, rather than wholesale abandonment of Stampe's notions of allophonic and morphophonemic (and consequently "phonemic" as well). Notice in this instance that both apparent cases of morphophonemic processes applying after the allophonic one are not ultimately morphophonemic processes at all in the sense originally defined by Stampe: i.e. processes introducing derivable features which happen already to occur in underlying representations (here meaning lexical or systematic phonemic form). Observe also that Glide Deletion in (27), like the Spanish deletion processes of various other orders mentioned in the final paragraphs below, can not be

either strictly morphophonemic or allophonic in Stampe's original sense, since it introduces no new "segments" at all (regardless of whatever neutralizations it may cause). T-Affrication, in turn, does not directly convert one underlying segment (t) to another (č); rather it converts an allophone (t') back to a phoneme (č), not at all the condition Stampe's definitions account for.

A similar demand for readjustment in our notion of "allophonic" versus "morphophonemic" derives from a careful inspection of the loanword data presented in Appendix Part Three. Of vital interest here are the presumedly allophonic processes of S-Aspiration (or S-Deletion) and Spirantization, operative in numerous loanwords like [sauwéh] "southwest", [yídiš] "Yiddish", [dihkáu] "discount", [elfúrfedéral] "The First Federal (Bank)", and others among TYPE THREE loanwords in Appendix Part Three.

Since the process of S-Aspiration is regularly operative among the processes which nativize loanwords in early stages of borrowing in MCS, an assumption would seem to be in order, in light of this and other criteria, that aspiration is indeed an identifiable "allophonic" process in MCS. Yet arguments for viewing S-Aspiration as morphophonemic rather than allophonic are, on the surface of it, equally impressive. If we can assume, say, that the [h] replacing /s/ as the output of this process is phonetically an approximation of underlying /h/ for this dialect which appears in [hugár] and [hirár] etc. (they certainly seem a reasonable "phonetic" approximation and I believe few Spanish linguists would reject such a suggestion), then we appear to be faced with a process of neutralization here. Of course, we could perhaps save the issue by being quick to point out that no neutralization is actually

occurring in this case (the essence of Stampe's formulation of "morphophonemic"), since the process in question occurs syllable-finally and the underlying segment /h/ is restricted to only syllable-initial position.[12] Yet the very point is that Stampe's original definition makes no mention of context whatsoever, only mention of the replacement of one underlying segment with another.

The active Spirantization process obvious in loanwords of MCS provides another and not altogether unrelated difficulty. It is reasonable to claim that the intervocalic spirantization of d, for instance, is somehow intimately realted to the frequent deletion of this same consonant word-finally (cf. Hammond 1976, pages 142-43). Are these not perhaps two manifestations, then, different only in degree, of a single obstruent weakening process? Yet if one is to accept a tentative hypothesis introduced by Hammond (1976, pages 237-42), that MCS may have lexicalized [-continuant] voiced obstruents, we would seem then left with the highly uncomfortable conclusion that one effect of a single process is morphophonemic while another manifestation is clearly allophonic. And if this is not true in the case of Spirantization, it would seem inevitably to be the case with S-Deletion, which can only be assumed a more extreme version (greater weakening) of the common S-Aspiration.

The productive native and loanword applications of processes like S-Aspiration and Spirantization would seem to suggest, then, need for a number of readjustments in the Natural Phonologist's conception of allophonic versus morphophonemic processes as teleologically distinct operations in any phonological derivation. Indeed, our conception of process types and process ordering must be expanded considerably beyond Stampe's early simplistic definitions of only two process types.[13]

NOTES

[1]Such observations, though they do exist, are rare however, most especially among the generative phonologists. Note, on the other hand, the following passage from Chafe (1968, pages 135-36):

> [It is] a possible hypothesis...that loanwords are, when they first enter a language, subject only to the persistent rules which are present in the borrowing language at Depth I [i.e. not strictly ordered before other rules]. These are, as it were, the rules of the language which are most alive. Older loanwords, like "haka" [gen. haan, "hook"] cited above, may in their own time have been subject to rules which are now at greater depths, and further assimilation into the borrowing language may even bring a more recent loanword under the operation of transient rules and rules at greater depths than I. But many loanwords will remain indefinitely subject to only those rules which were persistent at the time they entered the language (and to any rules subsequently introduced). Synchronically, of course, loanwords must be marked for their immunity to transient rules, to rules at depths greater than I, or the like [here read "persistent rule" as "allophonic process" and "transient rule" as unproductive or "dead phonological rule" -- PCB].

[2]For the full account of native informants see Appendix Part One. All radio taping was done from station WQBA and others on three separate occasions in January, August, and November of 1975. For further comments on fieldwork methodology see the concluding section of Chapter One.

[3]Fernández's unique study has provided an initial stimulus for much of the loanword data utilized in this chapter: i.e. my source for the loanword data discussed here was the pronunciations by native informants of selected loanwords found on the Linguistic Questionnaire reproduced in Appendix Part Two, and I have relied most heavily on Fernández's corpus of borrowings in selecting the items for inclusion on this original linguistic questionnaire.

[4]Fernández finds hybrid creations to form the second largest category among his sample corpus of borrowings. The total number of borrowings within several distinct categories was reported in that sample to be as follows (cf. Fernández 1973, page 77):

	Shifts	Hybrid Creations	Hybrid Compounds	Loan Translations
Formal Speech	18	5	3	2
Informal Speech	80	65	34	35
Formal Writing	18	3	3	5
Informal Writing	41	13	2	7
Total Borrowings	80	65	34	36

[5]It remains to be pointed out, then, the example given by Beardsley in illustration of this type (la sueda) is highly questionable and perhaps even inadmissible. This form seems in actuality to be one case of the speaker arriving at an acceptable underlying form in his native system through applying a paradigmatic process. We might assume that he is not able to interpret the English phonetic [D] within this word as either an underlying or derived segment in his own native system and therefore must lexicalize the word as is and apply a paradigmatic process to arrive at some underlying (phonemic) form (see note nine). On the other hand, it is a commonplace observation that Spanish-speakers (at least in Caribbean dialects) will take English [D] for their own alveolar tap [r]. At any rate, there are far better examples available of cases where the phonetic form of the borrowed item is acceptable as both an underlying and a surface form in the target system.

[6]Dalbor (1969, pages 128-30) provides one useful account of the distinctions between English and Spanish r and the allophonic varieties of Spanish r found in different dialects:

> In much of Spanish America the single voiced fricative [ɹ] and the single voiceless fricative [ɹ̥] alternate with or replace the tap [r] and the lengthened or doubled varieties do the same with the trilled [r̄].....In most of the Spanish-speaking world, however, including the area of General American Spanish, the tap [r] and trilled [r̄] are more widely used and enjoy more acceptability and prestige. Sometimes speakers in these areas use the voiceless trill [r̥̄] in place of the more standard voiced variety [r̄].....Americans usually have trouble with the trilled /r̄/ since this sound does not exist in American English, although it does in some dialects of British English, such as Scottish.....In any case, the typical English retroflex [r] sounds very strange to the Spanish ear and can even impede communication. It is, incidently, one of the last sounds mastered by Spanish-speakers, foreigners in general, and even English-speaking children themselves. English [r] is actually almost vocalic in its articulation and is often analyzed as a semivowel, a glide, a liquid, or even a lateral. But the Spanish /r/ and /r̄/ are purely consonantal.

[7]Notice that the loanwords formed in Fernández's list of hybrid creations are in fact all achieved by the application of morphological (word-formation) rules, involving verb, noun, and adjective suffixes. But there are also other types possible which are not strictly hybrid creations, e.g. those constraints (probably word-formation rules) requiring word-initial e before clusters of s plus consonant and requiring underlying lax final e on all nouns and adjectives whose surface forms do not terminate in an unstressed vowel. These rules are quite apparent in loanwords like hacer espí "to speed up" and sánwiche "sandwich".

[8]This particular example was called to my attention originally by Bohdan Saciuk. Notice that this interesting form is completely nativized through (1) regularizing of stress (phonological rule), (2) regularized plural formation (morphological rule), and (3) final stop deletion (natural process).

[9](19) seems a marked improvement over Ohso's original proposal that unacceptable foreign sequences will be "put in the lexicon in violation of the rule that prohibits them" but then made subject to that same rule (and thus eliminated from surface representation) at some later point in a derivation (1973, page 4). Also eliminated here is Hyman's Third Principle of borrowing (see page 321 above) which allows for the modification of environments in incoming foreign words to accommodate the applications of proper native processes. First, I found no clear evidence of such developments in my own loanword corpus. Also, Ohso has demonstrated that Hyman's own argument for such a principle is subject to a reasonable reanalysis. But perhaps most importantly, Lovins has revised this principle to read: "The 'environment' part of a context-sensitive process may be generalized in perception of a foreign sound sequence" (1974b, apge 244). What would seem to be involved, then, is a looser application of a native process rather than a different perception of what is an appropriate phonemic form for a borrowed word X or Y or Z.

[10]Notice that since this dialect (and Cuban Spanish in general) shows little if any tendency to voice /s/ intervocalically (cf. Haden and Matluck 1974, page 22), we can assume that there is no contextually restricted process s → z contradicting a dominant paradigmatic process z → s. It would be this latter process, then, which would account for the MCS perception of English [ríyzən] as /ríson/.

[11]I have no useful way to account for the vowel in the first syllable of this particular MCS form. Though there seems to be little motivation for any vowel other than /o/ (certainly the closest rendition of English [ow]), I have several invariant pronunciations of this word occurring on my tapes with the vowel as [a]. In fact, I have recorded or observed no other pronunciations for this form.

[12]Hammond (1976, page 247) reports that syllable-initial cases of S-Aspiration are observed in the Spanish of Puerto Rico and the Dominican Republic and it also now is heard in the speech of MCS. In these Caribbean dialects with syllable-initial aspiration of /s/, we would of course observe this process causing neutralizations of an underlying distinction, and S-Aspiration would therefore by Stampe's definition be accounted a morphophonemic process.

[13]Foremost, of course, the definition would have to be amended to account for actual cases of contextual neutralization as opposed to an application of processes affecting underlying segments in any non-neutralizing relationships. Still more vital will be the formulation of a new conception of the essence and role of deletion processes and their interrelation with that which is allophonic (non-neutralizing) and that which is morphophonemic (neutralizing). I have begun research in this area but as yet can report no significant conclusions.

Part Four

Syllabic Phonology

CHAPTER EIGHT

SYLLABIC PHONOLOGY AND NATURAL PHONOLOGY

> The syllable is a traditional, if often ill- or undefined, notion in phonological studies, though it is often mentioned only to be neglected.
>
> -- John Anderson

8.1 A Theory of Syllabic Phonology

Syllabic Phonology as an explicit and necessary modification of Generative Phonology is originally a contribution of Theo Vennemann (1971, 1972a) and an integral feature of a theory labelled Natural Generative Phonology which is proposed by Vennemann and subsequently extensively refined and extended by Joan Hooper (1972, 1973, 1975). The insufficiency of Vennemann's notion of "natural" in phonology, to date, has been that while Vennemann and Hooper advocate incorporation of syllable boundaries (Hooper 1972) and syllables (Vennemann 1972a, 1974b; Hooper 1973) as a construct in phonological descriptions neither ever formally establishes the absolute necessity of these concepts.[1] Without irrepressible evidence for the psychological reality of the syllable, Vennemann's grammar reduces to just another speculative and vacuous rational model.

This chapter, by way of contrast, is concerned especially with syllables and syllable boundaries as highly explanatory devices in phonological theory, most particularly the status of such formulations in regards to the recent discussions of weakening in segments (esp. Hock 1975, Matisoff 1975, Lass 1971, Gamkrelidze 1973) and the theory

of naturalness being proselytized by Stampe. Obliquely the issue of syllable-structure modification in the presence of borrowing is taken up. The following discussion is also designed to serve as a further commentary on the relative descriptive and explanatory merits of the Natural Generative Phonology of Vennemann (especially as it takes shape in the more explicit recent works of Joan Hooper) versus the Stampean conception of "naturalness" within phonological systems.

One of Vennemann's own early passages reveals the tentative status of claims for the role of the syllable in Natural Generative Grammar: "All phonological processes which can be stated in a general way with the use of syllable boundaries can also be stated without them, simply by including the environments of the syllabification rules in the formula" (1972a, page 2). Among advocates of Stampe's notion of Natural Phonology, Rhodes e.g. asserts mysteriously that syllable boundaries do not even exist in phonological representations (a notion which would inter alia wipe out Stampe's own analysis of divinity fudge) -- "...syllable boundary markers, like other boundary markers, exist only in rules and not in strings, representing an indirect limitation over and above any phonological limitations" (1974, page 295). Wojcik (1976) adopts an opposing view: processes remain sensitive only to syllable boundaries (productive morphology has the ability to create odd syllable boundaries which gives the appearance of morphological conditioning). Although little has to date been agreed upon by Stampean phonologists about the effects of syllable boundaries (or any boundaries) on processes, the theory must indeed be expanded in this direction if it is to confront pressing issues like the notions of strengthening and weakening phenomena.

James Foley (1970, 1973) contends that there are some types of assimilations which are not phonetic in nature and therefore must be given a purely phonological interpretation. The palatalization of /s/ before dental liquids but not before dental nasals in Norwegian is cited as one example (see Foley 1973, page 51). Here the narrow "phonological" interpretation apparent to Foley is that /s/ is strengthened in proximity to the stronger /l/ but not in the vicinity of weaker /n/.

On the basis of such evidence (and he cites other examples from across what turn out to be exclusively European languages) Foley motivates and proceeds to defend a scale of relative phonological strengths for phonological elements which takes on the following appearance of Figure Seventeen:

(1) FIGURE SEVENTEEN

FOLEY'S SCALE OF PHONOLOGICAL STRENGTH

t s n l w e

1 2 3 4 5 6

relative phonological strength p

where t is for oral stops, s for continuants, n for nasals, l for liquids, w for glides, and e for vowels

This tentative strength scale is intuitively phonological and not claimed on the basis of any phonetic properties, and as such it is immediately suspect for the concrete phonologist and most especially for the general phonetician. At the same time (and crucial even for the most abstract generative phonologist) there is also language-specific evidence that this phonological strength scale is simply inaccurate in most of its claims. That is, it can be shown to make

false predictions about the direction of the synchronic phenomena as well as about processes of phonetic change. That this is the case will be demonstrated here with evidence largely from Spanish, the language with which Foley himself has worked most extensively.

This chapter is concerned with motivating the concept of "phonetic" strength chains as an alternative to Foley's ingenius but ultimately unworkable system of purely "phonological" strength. One purpose will be to show that some of the phenomena Foley cites from Germanic (e.g. the retention of schwa after dentals in third person singular verbs, or monophthongization of au to ō before dentals) as being "uninterpretable in a phonetic system" do indeed have a natural phonetic explanation within a system of Natural Phonology as proposed in the works of Stampe. A larger issue will be a claim here that a developing theory of strength chains based on pure phonetics (i.e. articulatory or acoustical simplification) is sufficiently capable of explaining a wide range of assimilatory behavior among the segments mentioned by Foley and Hooper, along with other synchronic and diachronic phenomena as well.

The argument advanced here is based largely on a notion of formal "weakening chains" or "target chains" in Natural Phonology, a concept first applied by Larry Nessly (1973) and one of the direct outgrowths of the theory of Natural Phonology being advanced piecemeal by Stampean phonologists. The chapter makes three controversial claims: (1) Foley's purely phonological scales of relative strength have little if any empirical support and are, in fact, repeatedly contradicted by the evidence from Spanish; (2) the purportedly phonological and phonetic consonantal strength hierarchies which are part of Hooper's

system of Natural Generative Phonology err primarily in her assumption that positions of phonological strength are statable exclusively or even prominently in terms of the syllable; and (3) the phonetic weakening chains suggested originally by Nessly (and Rhodes and Pyle) provide the most desirable account of relative synchronic strengths of segments as well as the most supportable explanations for linguistic change. A consideration of Hooper's arguments and the Spanish data on which they are based reveals that Natural Generative Phonology fails to handle with its Positive Syllable Structure Condition a necessary distinction between syllable-initial and word-initial positions as conditioning factors on synchronic phonological processes. Diachronic developments in the historical evolution of Spanish grammar are also cited as relevant. And although the theory of weakening chains proposed by Nessly deals with vocalic as well as consonantal weakening, the discussion in this final chapter will be largely limited to the purely consonantal phenomena of Spanish and of Romance.

8.2 Consonantal Strength Hierarchies

This section is largely a summary and evaluation of proposals about phonological strength scales made previously by Foley (1970, 1973), Hooper (1973), and Nessly (1973). A fuller treatment of these proposals can be found, of course, by consulting the originals of these studies.

Foley's theory of relative phonological strengths outlined above (Figure One) is, from what he tells us, based on two kinds of evidence, both with highly-questionable status. First, there is the fact of an assimilation like the reported s to š defore dental l but not n in Norwegian. As Foley observes, the palatalization of a dental before

another dental appears at first more like dissimilation than assimilation. Foley's initial assumption, and one which we will shortly dismiss as contradicting the phonetic evidence, is that the conversion of s to š is a case of phonological strengthening. The palatal continuant is stronger than the dental continuant only in terms of Foley's second scale of relative strengths (given as Figure Two below) which suggests that the dentals are always stronger than either velars (the weakest segments) or labials (intermediate as to phonological strength).

Foley also presents questionable evidence for superior phonological strength of the segment l over n. This is supposedly found in the fact that an Indo-European cluster ln regularly assimilates to ll. (E.g. Kiparsky 1973a, page 15, notes that the generalized rule n $\rightarrow C_i$ / C_i___ is a natural process accounting for English kiln → [kil], Latin *colnis → collis, Greek *olnūmi → ollūmi etc.) We find Lithuanian pilnas and kalnas cognate to English full and hill from the Latin plenus and collis. Here the direction of phonological strength proposed by Foley (laterals stronger than nasals) is contradicted by at least the evidence from Spanish; strength scales developed by Hooper (1973) to account for conditions on Spanish syllable structure and claiming to have a universal application show the nasals as a full step stronger (see below) than l and r. Foley calls our attention to an apparent paradox that n is found in an apparently stronger syllable-initial position in forms like the Lithuanian pilnas and kalnas, hence suggesting that the best case for a relatively stronger status for l is precisely that it overrides n even when n has occuppied the recognized position of superior phonological strength.

In order to find a more reasonable solution to this possible paradox of strengths, and at the same time denigrate Foley's narrow

interpretation, we need only look as far as James Hutcheson's recent observations that for many consonantal assimilations, at least, the change is predicated on the complex phonetic make-up of the individual segments, and that this is most notably true when assimilations involve some degree of sonority. Hutcheson offers evidence that there is an attested hierarchy of relative sonority as well as relative strength in consonant articulations and that less sonorous segments than liquids assimilate readily to the liquids with which they already share other common features -- like the point of articulation (cf. Hutcheson 1973, pages 34-5). The appropriate example (Hutcheson cites Buck and Hale) is that a rule which existed in Latin assimilated -dl-, -ld-, -nl-, -ln-, -ls- in all cases to ll and -mb-, -nd-, -ng- to mm, nn, and nn respectively. We will return to a further discussion of apparent misdirections in Foley's proposal about strength and the direction of these assimilations in the sections below.

The particular point of Foley's article seems to be that a scale of purely phonological strength like that in (1) is necessary to account for what he takes to be only three examples among many of "non-phonetic" phenomena occurring in the vicinity of Germanic dentals but not in the environments of Germanic labials or velars. The examples cited by Foley are as followings:

(2) In Anglo-Saxon z becomes r before dentals but not before the other obstruents (e.g. huzd gives hord "treasure" and razda gives reord "speech").

(3) The thematic vowel of the 3rd person singular verbs in modern German elides after labials or velars but not after dentals (bebt for *bebet, sagt for *saget, but arbeitet and wartet).

(4) The Germanic diphthong au always monophthongizes to ō when (but only when) followed by a dental (e.g. Gothic daupus gives German Tod and hausjan yields hören, but augo remains Auge and hlaupan gives laufen).

In each case Foley's hasty conclusion in relation to these alternations is that there remains no shread of evidence for any phonetic explanation of the diachronic or synchronic segment changes.

Foley next presents three additional examples in support of a contention that for Germanic the dentals are the strongest among occlusives. These examples are (1) changes observed in a well-established High German Consonant Shift, (2) a shifting of final m to n in Germanic, and (3) a shift from English t to ʔ after stressed vowels and before the syllabics l or n.

To take up first the High German Consonant Shift, here we find that all dentals undergo shifts, some labials are subject to this change as well, but no velar occlusives are involved in such shifting. Relevant forms would be:

(5) HIGH GERMAN CONSONANT SHIFT

t	→	ts	tooth/Zahn
d	→	t	door/Tur
p	→	pf	pipe/Pfeife
b	→	idem	bed/Bett
k	→	idem	corn/Korn
g	→	idem	grave/Grab

The point Foley wishes to draw from this is that it is precisely the inherently strong segments that will be most apt to show further strengthening, while it is the weakest segments which are susceptible to the earliest as well as the most extensive weakening. It is from

the evidence of the High German Consonant Shift, furthermore, that Foley deduces his second relative phonological strength scale -- this one to indicate strength exclusively among the occlusives.

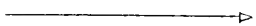
(6) FIGURE EIGHTEEN

FOLEY'S SCALE OF GERMANIC OCCLUSIVE STRENGTH

k p t

g b d

ŋ m n



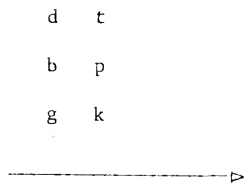
1 2 3

relative phonological strength x

Two final examples supporting the strength of dentals in Germanic are drawn from changes in the strong position for segments. Here we have claims based on an assumption that when a first consonant is replaced by a second in strong position it is the second consonant which demonstrates the greater strength. Foley gives us the case of Germanic Auslautsverhärtung (or consonant strengthening in final position). Since we find Old Latin quom and tum (and other like forms) cognate to Gothic h^{v}an and pan etc., evidence would seem to be, under this principle at least, that the dental n is the strongest of the nasals. Foley contends that the popular case of German final consonant devoicing is as well a parallel example of Auslautsverhärtung which motivates a third relative strength scale for the Germanic consonants. This scale would cature a supposition that voiceless occulsives are stronger than the voiced variety, since the preferred direction in final (= strong) position is towards devoicing.

(7) FIGURE NINETEEN

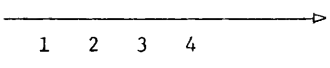
FOLEY'S STRENGTH SCALE FOR GERMANIC STOPS



The final example of conversion from English t to ? is also based on the assumption that change in strong position is a change from weaker to stronger segments. Yet here an additional assumption has to be made: if the consonant subject to the strengthening is already a segment of greatest possible strength, then the only possible change (presumably still a form of strengthening) is a change back to the weakest segment on the scale. When English t becomes ? in a strong position following a stressed vowel and preceding a syllabic l or n, Foley concludes that here we have evidence of just such a case of strengthening manifested in the appearance of the weakest of conceivable related segments.[2] English forms in which the change occurs are given as fountain, mountain, Latin, bottle, kitten, mitten etc., where the dental stop undergoes such a shift, as opposed to, say, beckon, pickle, nipple, and weapon, where labial or velar stops are not subject to it. This final example convinces Foley to revise strength scale (6) as (8) [showing here only the string of voiceless stops -- PCB].

(8) FIGURE TWENTY

FOLEY'S REVISED SCALE OF GERMANIC OCCLUSIVE STRENGTH



relative phonological strength x (revised)

Let us review at this point three exceedingly strong claims made by Foley's theory of purely phonological strength and to which we will want to return for discussion in the concluding parts of this chapter. These are as follows in (9) through (11).

(9) Changes of the type illustrated here by (1) the Anglo-Saxon replacement of r for z before dentals, (2) the failure of elision of thematic vowels after dental stem-final consonants in German, and (3) Germanic au monophthongization all have no phonetic basis and must therefore be accounted for within a powerful theoretical framework which includes a concept of relative phonological strength.

(10) If consonants undergo change in the phonologically strong position the change will always be a change from weaker to stronger segments (that is, strengthening is coterminous with labelled "strong position" and we would not expect, in Foley's system, where final position is "strong", change from final devoiced to voiced obstruents -- which is exactly what happens, of course, in those Germanic dialects which have lost a well-known final devoicing process.

(11) If a consonant being strengthened already possesses a status of greatest strength, this consonant will be expected to convert to the weakest possible segment as a normal manifestation of its strengthening. I will want to contend below that proposals

of these types are all susceptible to serious objection, once we make more careful analysis of the data at hand.

Before taking up a more complete argument against Foley's theories, let us look briefly at two additional contrary proposals for capturing a conception of relative phonological strengths. The first is found in Joan Hooper's (1973) recent dissertation proposing a highly constrained phonological theory which (after the model of Vennemann) she has been labelling a Natural Generative Phonology. Hooper's own version of strength scales are part and parcel of an attempt to replace what have proven inadequate notions of sequence structure conditions in the standard generative theory (i.e. morpheme structure conditions and lexical redundancy rules, as treated in Chapter Four above) with a program for choosing the syllable as the basic unit for stating phonotactic constraints in a "natural" generative grammar.

Chapter Nine of Hooper 1973 presents a Consonantal Strength Hierarchy for Spanish which is also a candidate for a universal strength hierarchy (Chapter Ten) and which has the purpose of establishing what Hooper calls the Positive Condition on Spanish syllable structure. This Strength Hierarchy and related Syllable Structure Condition are stated in Figure Five and Figure Six below. I have repeated both diagrams as they appear in Hooper's work in order to facilitate subsequent discussion, where I will take up two (among several) unworkable aspects of Hooper's mechanism for conditioning syllable structure in terms of phonological strength.

(12) HOOPER'S SCALE OF SPANISH CONSONANTAL STRENGTHS

							f		
			m		ƀ	ŷ	b	p	
y			n	s	đ	r̄	d	t	č
w	r	l	ñ	x	ǥ	ǥw	g	k	
1	2	3	4	5	6		7	8	

tentative scale for American Spanish casual speech

(13) HOOPER'S POSITIVE SYLLABLE STRUCTURE CONDITION FOR SPANISH

P(C): $\$C_mC_nC_pCV_qC_r\$$

where $n \leq 3$

If $n > 1$, then $m \geq 6$

$m > n$

$p,q = 1$

$r \leq 5$

$n > p$

$r > q$

Interpretation (possible distribution of consonants in any syllable of Modern Spanish)

m1 = f, p, t, k, b, d, g

(m1 = another C may follow)

m2 = s, m, n, ñ, l, r, r̄, č, x

(m2 = no other C may follow)

n = r, l

p,q = y, w

r = s, m, n, l, r

A Positive Condition for Spanish syllable structure follows, then, from the observation of definite hierarchies governing the suitabilities of given consonants for syllable-initial and syllable-final positions. The preference scale for segments syllable-initially is as follows (in descending preferential order): obstruents, nasals, liquids, glides, vowels. Syllable-finally the apparent preference is reversed: vowels, glides, liquids, nasals, obstruents. This is a similar generalization available everywhere in the literature on phonetics. The hierarchy of preferences is perfectly consistent, notice, with the frequent observation that a favored syllable structure for Spanish is clearly CV and that the only reasonable syllabification of the cluster VCV would be V$CV (see Hooper 1973, pages 146-7).

Hooper's strength scale (and likewise its application to syllable structure) has a degree of explanatory as well as descriptive power, in so far as this reading of consonantal phenomena is verified in the work of earlier phoneticians and phonologists. Hooper (1973, page 110) cites Jespersen's proposal among others that groupings of sounds in the syllable are strictly patterned according to sonority and audibility. More sonorous segments fall at the nucleus; less sonorous segments fall farther and farther from this nucleus. Jespersen's ranking of sonority is repeated by Hooper in something like the form of Figure Twenty-One.

(14) FIGURE TWENTY-ONE

JESPERSEN'S RANKING OF SONORITY

LEAST SONOROUS

1. Voiceless Consonants

   (stops: p, t, k)
   (fricatives: f, s)

2. Voiced Stops

   (b, d, g)

3. Voiced Fricatives

   (v, z)

4. Nasals and Laterals

   (m, n, l)

5. Trills and Flaps

   (r)

6. Closed Vowels

   (y, i, u)

7. Mid Vowels

   (e, o, ɛ, ɔ)

8. Open Vowels

MOST SONOROUS

   (a)

Another way to schematically capture the conditions on syllable structure is to represent the syllabic unit as one whose nucleus is the most vowel-like portion and whose outer fringes will be the least vowel-like. The resulting representation would rest on considerable phonological evidence (Hooper 1973, Chapter Seven, for discussion) that universally it is the syllable-initial position that is the position of greatest individual segment strength.

(15)

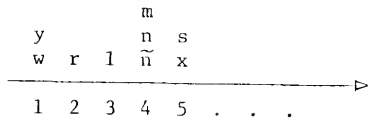
ONSET ·····				PEAK				····· CODA
obstruents	nasals	liquids	glides	VOWELS	glides	liquids	nasals	obstruents
least vowel-like				most vowel-like				less vowel-like
STRONGEST				WEAK	WEAKEST	WEAK		STRONG

The phonetic basis for consonantal strength hierarchies reduces to a claim that increased phonological strength has most directly to do with reductions in the amount of consonantal energy produced (cf. Hooper 1973, pages 119 and following for a discussion of proposals to this effect from Jakobson and Halle). Yet Hooper's syllable condition and proposed strength rating is not in itself completely "phonetic" in nature.

There is, for example, the difficulty in accounting for relative weakness in the segment s. The only apparent evidence for this proposed weakness of s in Spanish is the totally circular evidence of its distributional properties within the Spanish syllable. The weakness of this phone in Spanish, furthermore, is at odds with the considerable strength of this same segment in, say, English. Hooper (page 144) suggests that the relative stability of s compared with the voiceless stops in syllable-final position has to do with an appropriateness for

this position assigned to the segment by the Spanish strength scales. But the behavior of English s in syllable-initial clusters indicates again that for that language the segment is noticeably strong. The conclusion hit upon (see Hooper, page 144) is that s is potentially quite flexible as to its strength and that this is probably due to certain phonetic properties -- coronality, continuancy, stridency -- which make it compatable with pre-nucleus positions as well as post-nucleus positions and before stops. But again, the flexibility of s in English is not matched by any similar flexibility in Spanish. And no real phonetic evidence or physiological properties have been brought to bear on any accounts of the segment.

The Positive Syllable Condition and strength scale for Spanish represent, in Hooper's view, a major economy and highly significant generalization in the grammar of Spanish, since they account for such synchronic and diachronic phenomena as syllable-initial strengthening or the process of syllable-final weakening, and this at no additional cost. Here we may best quote a longer passage as both evidence and example.

> Consider the SSC [Syllable Structure Condition -- PCB] posited for informal Spanish in which r = 5. Both historically and in formal speech, the syllable final /p/ appears in concepto. For casual speech, the /p/ must have a value of 5 or less. An appropriate change of features must take palce. Of course the goal is to change as few features as possible. The result of the weakening must have a large number of features in common with /p/, and apparently a choice must be made out of the finite inventory of phones occurring in that dialect. If r = 5, one of the following would be weak enough:
>
> 
>
>
> The features that must be changed in order for /p/ to weaken are precisely the features that make /p/ strong: [-voice], [-sonorant] and [-continuant]. The identifying feature that will be maintained is the point of articulation feature, identified by the cover feature [+labial]. By this reasoning, both [w] and [m] would be

acceptable choices. Because of the nature of the nasality, weakening of obstruents is never manifested through changing [-nasal] to [+nasal] unless a nasal segment is contiguous in the sequence. Therefore the only choice is [w]. Given the following feature specification for [p] and [w], the features of [p] may be changed to the features of [w] by calling upon the universal redundancy rules discussed in Chapter 8:

	p	w
sonorant	-	+
voice	-	
continuant	-	+
labial	+	+

The particular redundancy rules that will be called into play depend upon the extent of the weakening, the phone inventory of the language and the language specific strength hierarchy (1973, pages 151-2).

Syllable-final weakening in Hooper's model can therefore be handled with little if any extra "machinary" appended to her version of the grammar. There are no independent rules for weakening. All that will be necessary to account for the process are (1) the Positive Syllable Structure Condition for the given dialect, (2) the relevant strength scales, (3) a phonetic inventory, and (4) universal feature redundancy rules (page 152). The syllable-structure condition and a related cover feature of strength -- claimed to be essentially universal and also independently motivated -- account in this fashion for at least most constraints on sequence structure in Spanish, as well as allophonic variations and diachronic change that is conditioned after one fashion or another by syllable position. Strength chains for Hooper, then, have a central role in the synchronic as well as diachronic grammar for Spanish.

A final approach to relative phonological strength is the approach of Natural Phonology represented in Nessly's (1973) limited work on "weakening chains" and "target chains" as diachronic phenomena. These processes entail systematic weakening and eventual deletion of segments

in certain special environments and are characteristic of both consonantal and vocalic segments. Let us list first some advantages of an alternative proposal.

The achievement of the "weakening chains" approach over the others illustrated so far is that (consistent with general goals of Natural Phonology as an explanatory rather than purely descriptive enterprise) this approach replaces the attempt at comprehensive description of all available linguistic phenomena (i.e. strength scales) with the more limited but more useful elucidation of a restricted set of data that most readily lends itself to explanation. A difficulty with Foley's theory of non-phonetic strengths is precisely that it does not account (as it purports to do) for all cases of consonantal change, even among the limited examples available in his brief article. We will discover presently that Hooper's thesis is also similarly restricted despite claims of universality. A theory of weakening chains makes no outrageous claims about changes not amenable to a physiological explanation. What this Natural Phonology approach gives up in scope it would seem to gain back severalfold with its deeper insight into linguistic change and its wider compatability with empirical evidence.

Nessly cites selectively from the bulky evidence that consonants weaken and eventually delete intervocalically. Some of this evidence is of the following kinds. The voiceless stops quite generally become voiced between vowels (Nessly cites examples from Danish and Spanish taken from Foley). A closely related phenomenon is that voiceless fricatives display a similar tendency in this position, as evidenced by German and Italian (1973, page 467). Voiced stops becomes fricatives in this same position (examples are again those of Danish and Spanish

brought up earlier by Foley). And finally, fricatives show the strong tendency to delete in this intervocalic environment, with the most familiar case being perhaps again that of Spanish.

Two other factors in this general historical and synchronic process of consonant weakening should be carefully noted. It seems evident from the cross-language data that weakening of stops to spirants occurs along a preferential scale from velars (the weakest segment) to dentals to labials (compare generally the work of Gamkrelidze). Velars are usually the earliest to delete. Furthermore, the three relevant processes in consonant weakening -- voicing, spirantization, and deletion -- apply in serial relationship: i.e., it is only the voiced obstruents that will spirantize and it is then the fricatives which are generally subjected to deletion.[3]

The environment for the weakening of vowels is more general and in fact each of three related steps of weakening -- reduction, devoicing, and deletion -- shows its own peculiar environment. Nessly (page 467) cites work particularly by Bell on vowel deletion which encompasses all of the following observations. High vowels and reduced vowels are the most readily lost while accented vowels are rarely so. The pre- and post-stress positions often will favor vowel loss. Next to sonants and between identical consonants are also positions especially susceptible to vowel loss. With respect to devoicing, on the other hand, it is the vowels of weakest stress, short length, and low pitch that are most prone. Also, high vowels are especially compatible with devoicing, and utterance-final position is the most favored location. Finally, Donegan Miller's (1972a) extensive work on vowel systems is also cited as evidence that vowel reduction is most probable with the unstressed

non-tense vowels, with low vowels rather than high vowels, and with open medial syllables rather than closed syllables.

The central issue which Nessly wishes to raise for phonological theory, and which he is at great pains to make with lengthy discussion of processes we have sketched in the above two paragraphs, is just that separate facts of vowel weakening and consonant weakening have more in common than should be assigned to mere coincidence. Since generative grammar strives to relate similar rules -- either formally through some notational convention or functionally by positing rule "conspiracies" -- in order to capture the significant generalizations of any grammar, it would appear desirable in a similar fashion to relate the subparts of these diachronic weakening processes.

It is possible and even reasonable to assume that these related processes of consonant weakening and vowel weakening are encompassed by mechanisms already available within the framework of generative grammar. Intervocalic consonant weakening, for one thing, may well seem to be simply a case of "assimilation" -- as it has usually been treated. Nessly (page 469) himself raises this question whether weakening may indeed be an assimilation to nonconsonantal (i.e. weaker) features of surrounding vowels and responds to the hypothetical challenge that such an explanation would have difficulties with the cases in which there is full deletion of segments. I will take up this question of weakening versus assimilation again briefly below and will thus leave it aside for the moment. A more serious challenge to the theory of weakening chains may be that consonant weakening and vowel weakening are brought on by a conjunction of processes that are accounted for elsewhere in the literature as a "conspiracy" of phonological rules.

Of course, we would be speaking of a universal conspiracy here (something not provided for in the literature) as the various consonantal phenomena, for example, of devoicing, spirantization, and deletion are found to be operative across many languages and are not necessarily found to be contemporary processes of any single language. But there is a further sense (pointed out in Nessly's paper and apparently suggested to him first by Charles Pyle) in which weakening chains are not functionally conspiracies in the normal sense. Nessly notes the following difference as basic:

> Weakening chains are different from traditionally collapsed rules, universal rules, and conspiracies, and cannot be expected to be describable in terms of those analyses, since all of these phenomena have different principles of motivation. A universal rule is motivated by the differential susceptibility of sounds to phonetic processes; a conspiracy is motivated by the attempt to produce a given output; and a weakening chain is motivated (particularly in vowels) by an attempt to diminish the articulatory prominence of a sound that is given diminished structural prominence. The motivation in neither of the weakening chains is one of deleting sounds, but is rather one of reducing articulatory effort in producing non-prominent sounds (page 474).

By focusing on underlying principles which produce the surface effects we are referring to here as "weakening" a number of possible contradictions are eliminated and the explanatory power of our theory is increased. Let us return briefly to the observation that a first stage of consonant weakening is the voicing of segments in intervocalic position. We have already made mention (passim) of the Germanic process of final obstruent devoicing, and it is also a familiar phenomenon that final obstruents, particularly the tense obstruents, undergo a process of devoicing across many languages. Since syllable-final position, at least, is apparently a position of greater weakness (Foley notwithstanding), and since this devoicing process represents a process of strengthening (segments become less vowel-like), apparently conflicting tendencies seem to be in operation. But this is somewhat

misleading and in the terms of Natural Phonology there is ultimately no contradiction at all. Obstruents would tend to become voiceless regardless of context since their oral constriction is an impedement of air passage required for voicing. But intervocalically, in voicing contexts, it is the weakening through process of assimilation that becomes the easier articulation. Weakening is a restricted contextual phenomenon and it is at this point helpful to recall Nessly's observation that a goal is not a weakening or deletion (in itself) but the reduction of articulatory effort in pronouncing segments which have a reduced structural prominence. We will turn to a few further examples of weakening processes as physiologically motivated in the subsequent section, which we now address.

8.3 Weakening Chains in Natural Phonology

The next section of this chapter will comprise some notes on the several inadequacies and inaccuracies involved in both Foley's and Hooper's proposals governing phonological strength. Some additional evidence will also be offered for the soundness of Nessly's speculation about the class of weakening chains. What I want to be suggesting here is that no comprehensive theory of "strong/weak" based strictly on abstract phonological criteria is feasible in view of our present state of knowledge. The best we can hope for, at present, is a detailed phonetic account of those many processes which are less stingy in revealing their actual physiological origins.

First I will return to Foley and take up the strong claims of his article outlined in the previous section. We begin with the first example given of an apparent strength in dentals which conditions phonological change.

Recall that in Anglo-Saxon early Germanic z has shifted to r before

dentals (e.g. huzd ▷ hord) and Foley sees this to be indicative of a strengthening process. There is no phonetic reason to account for the Rhotacism of z here, Foley contends, yet (so the argument goes) the phonological manifestation of z-Strengthening would inevitably be r. I will not broach the issue of the phonetics of Rhotacism but instead will consider the initial assumption that dentals are of greatest potential strength. If the dentals are taken as strongest in Germanic, as Foley proposes, this is certainly not a universal categorization. The counter evidence I select comes from standard Spanish.

A general condition on syllable structure in Spanish (and perhaps universally) as formulated by Hooper (1973) is that a first member of any syllable-initial cluster must be considerably stronger than the following member (see (13) above, viz. condition m ▷ n). We can observe that among other impermissible clusters Spanish does not allow */tl/ and */dl/ syllable-initially, though /tr/ and /dr/ are common. The only successful account of this of which I am aware presently is Hooper's solution (1973, page 134) that dental stops are considerably weaker than either labial or velar stops while at the same time /l/ is stronger than /r/ (see (12) above).[4] Clusters */tl/ and */dl/ are not permissible clusters if only because a difference in strength between these dentals and /l/ is not sufficient for clustering.

There is other impressive evidence for the weakness of dental stops in relation to labials and velars. /d/ is most commonly deleted intervocalically in rapid speech. Latin T between vowels voices to d in Spanish and Portuguese (though not in Italian) while Latin D is dropped regularly for French and Portuguese and irregularly for Spanish. Final /d/ in Spanish (usted, edad, verdad, Madrid, etc.) has weakened

almost to a point of inaudibility and pasao for pasado "past" or soldao for soldado "soldier" are not uncommon in most modern Spanish dialects. Hooper notes that in fact the only strong obstruent occurring in syllable-final position regularly in Spanish is [đ], and also among nasals it is the coronal articulation that is favored word-finally (1973, page 139). It would seem that the entire coronal series (t, d, đ, n, s) is weakest in Spanish, though in English (viz. a Germanic language) the coronal series is more justifiably seen the strongest, permitting syllable-final clusters like /pt/ or /kt/ but not */tp/ or */tk/.

A second example of strength in dentals given in Foley 1973 has to do with the elision of thematic vowels preceded by stem-final labial or velar obstruents but not by dentals. Here it is assumed that ə has assimilated to the strength of the preceding strong dental becoming ə+ in arbeitet (3rd singular, "he works") and avoiding the deletion as we would have it in bebt or sagt or folgt (beben "shake", sagen "say", folgen "follow"). There is, however, a ready-made explanation here that has little to do with phonological strengths. The issues seem rather to surround the pressures for semantic coherency and paradigm regularity overriding the pressure of phonological processes. The blocking of the schwa-deletion in this particular example is quite evidently motivated by a necessity of maintaining a distinct morphological unit -- the person/number marker of the conjugated verb. In the cases of the stems with final t (arbeit-) a deletion of schwa would result in complete merger phonetically of the stem-final segment with the p/n marker and the semantic and paradigmatic identity of the form would be lost, a less than desirable state of affairs in the grammar. No such loss occurs in sagt or folgt, where the stem-final and inflectional segments remain

distinctly audible without the intervening schwa.

A third putative example of the strength of dental segments cited by Foley is the historical monophthongization of au before dentals but not before labials or velars (daupus/Tod but augo/Auge). This time Foley proposes that the diphthong first achieves the strength of the following strong consonant and that this increased strength then binds the separate units into the single monophthong. But here again it is most unfortunate for Foley's case that evidence would indicate monophthongization of this sort is a process not related in any clear sense to the qualities of phonological strength. There are examples that the monophthongization, first off, was not restricted to a dental environment (e.g. Gothic hauhs "high", Old Bavarian haoh, Old German hōh). What is really involved here is monophthongization of what Patricia Donegan Miller's work has been identifying as sonority-color diphthongs as opposed to mixed-color diphthongs, which results from a context-free process lowering the labial element of the original diphthong and an eventual assimilation between vocalic elements, and which has nothing to do with a surrounding consonantal environment.[5] A passage from Stampe (1972, pages 588-89) makes this altogether clear:

> It appears that, unlike monophthongization of mixed-color diphthongs, monophthongization of sonority-color diphthongs always occurs through assimilation. This is why is dialects which monophthongize a͡i, o͡i, ae͡u (buy, boy, cow) to ā, ō, æ the glides of e͡i, o͡u (bay, bow) remain; in the former diphthongs the glides were first assimilated to the low syllabic (æ͡e, o͡e, æ͡o). This view is supported by the fact that while many Germanic dialects monophthongized a͡i, a͡u, German and Old Icelandic monophthongized them only in contexts which lowered i͡, u͡ to e͡, o͡: Gothic hauhs "high", 8th c. Bavarian haoh, later OHG hōh, OIce. hor; Gothic air "before", 8th c. Bav. aer, OHG ēr, OIce. ār. The same facts confirm the lowering effect on the syllabic by the low nonsyllabic: while in Old Icelandic syllabic i, u were lowered in these contexts, in German they were not; since non-syllabic i͡, u͡ should be more resistent to lowering, because of their intense coloration, than syllabic i, u, we must conclude that in German not only the following consonant but also the low syllabic was responsible.

A word is also in order about Foley's example of Auslautsverhärtung. Here it is claimed that changes occurring in strong position are evidence for relative strength among consonants. Specifically, consonants in strong position are expected to strengthen but never weaken. Foley remarks that a usual example of Auslautsverhärtung is the overworked German final devoicing rule, in as much as the devoicing is a change from weakness to strength in final position (yet notice the internal contradiction here in Foley's system, where vowels are strongest!). I would counter at this point that final devoicing is more properly viewed a natural process in the sense defined by Stampe and therefore results from the principle that articulation of voiced obstruents is an articulatory difficulty to be eliminated wherever possible. If we assume with Foley that final devoicing is a "normal" development toward strengthening in final position (rather than a manifestation of efforts toward simpler articulations), how then are we to sufficiently explain away those cases (Swiss dialects of German) where this process seems to be rather systematically lost? Such a suppression of natural processes, we have seen, is not a difficulty within the Stampean theory of Natural Phonology, but Foley gives no principle by which strengthening ("phonological" strengthening) in strong position should be "naturally" suppressed. Note one additional difficulty with Auslautsverhärtung. In discussing the English conversion of t to ? in strong position (again conversion of the "strong" dental) where labial p and velar k do not change, Foley argues that only the strongest segment on the relative strength scale (here Figure Four) is subject to change and less strong segments like p and k must remain stable. If such might be the case, it is not apparent then why in the historical transition from Latin quum to Gothic $h^{v}an$

we find a reconstituting of m, which occupies the same position on this strength scale (see Figure Two) as does p. In strong position in forms like English nipple we find that strengthening is blocked for segments of Phonological Strength 2 (Figure Two); but in forms like Latin quum this is not at all the case. And for this (at least as Foley presents it) there is no principled explanation.[6]

What should by now be apparent about "strong/weak" designations in consonants from the discussion up to this juncture is the extreme complexity and irregularity of the multiple phenomena involved. Hutcheson in his dissertation (1973, Chapter Four) underscores some essential elements and hidden facets of this complexity, suggesting for instance that Grammont's classic principle (that Strong assimilates Weak to itself) is a scientific notion of strength hierarchy far too loose to be of any real service to the phonologist. Hutcheson also notes that phonological strength may have its origin in any number or combination of a considerable list of factors. For one thing, weakness or strength may have to do with the individual features of segments rather than individual segments, and some segments may therefore simultaneously show properties of strength and weakness with respect to their different features (this situation exists in relation to nasal assimilation rules -- see Hutcheson, page 83). Also, as mentioned, there is a hierarchy of sonority of segments and thus more sonorous segments tend to be the stronger with respect to general assimilations (Latin dl, ld, nl, etc. all become ll). Or some segments, like nasals, or especially the dental-alveolar nasal, are particularly prone to shifts in position of articulation. Phonetically motivated assimilations also result at times from "sequencing difficulties" between segments (here see Hutcheson's treatment of apical plus non-apical

clusters on pages 13-20). A final additional factor determining the strength or weakness of a segment in assimilations is the position within a syllable. In brief, then, "strong/weak" distinctions derive from a number of often unrelated sources -- the phonetic make-up of the segment, the sequencing of segments, location within the syllable, and also even the non-phonological factors such as interferring rules of morphologization (this last possibility obviously being one not appropriate to our treatment here; therefore see pages 93-102 of Hutcheson's discussion).

8.4 Strength and Spanish Syllable Structure

Hooper's work (as briefly reviewed immediately above) is sufficient to suggest that syllable make-up is a major determinant of phonological strengths of segments and that, in due turn, segment strength or weakness is largely responsible for the character of the individual syllable. But we have reached little agreement on which is more basic or causative. Is "strength" a motive for certain syllable structures or vice versa? Therefore in this final section we will look briefly at some exemplary failures in a possible approach to syllabic phonology (viz. Natural Generative Phonology) which ignores issues raised by notions of "weakening chain" as a defining feature in diachronics.

One most apparent flaw in Hooper's system has to do with the treatment of a regular Spirantization process. It is a cardinal principle of the Positive Condition on syllable structure devised by Hooper that syllable-initial position is a position of greatest strength and therefore a position appropriate for consonantal strengthening. Yet the Spirantization for voiced obstruents in Spanish is a weakening process of the synchronic grammar which regularly occurs in syllable-initial position (e.g. [pá$ðre], [lá$ɣo], [wé$βo] "huevo"). This happens to

be the case, of course, because such Spirantization is an intervocalic process and Spanish (all dialects) VCV clusters will always be subsequently syllabified as V$CV. Hooper must therefore at the outset weaken her stand on the role of syllable-initial position in determining segment strengths to the compromising claim that although syllable structure is an important conditioning factor in phonological processes, it is often overridden by other competing contextual factors.

The Positive Syllable Structure Condition also fails in light of a necessary distinction between syllable-initial and word-initial positions as conditioning factors on phonological processes. Once again we can take notice of an often-cited diachronic development in Spanish grammar. The Latin stops P, T, K in intervocalic position weaken through voicing in the Vulgar Latin period and appear ultimately as Spanish b, d, and g (before a, o, u) (e.g. sapēre/saber, matūru/maduro, fricāre/fregar, etc.). Yet in word initial position or adjacent to another consonant no such historical weakening occurs (e.g. pacāre/pagar, propriu/propio, trēs/tres, jūstu/justo, casa/casa). A parallel condition and what is obviously the following step of the weakening chain is at the same time observable with the Latin voiced stops: B, D, G in word-initial position are conserved by Spanish (e.g. bonu/bueno, gurdu/gordo); yet intervocalically, while B is generally stable, G is lost at times when in contact with e or i, and D regularly deletes (dēbēre/deber, plaga/llaga, lēgāle/leal, crēdit/cree). What is problematical here for Hooper (n.b. she actually discusses the synchronic but not the diachronic weakening rules) is that all these positions are syllable-initial and all are thus presumably subjected to the same treatment under her scheme of conditioning change through syllable structure. The necessary modification for Spanish would

seem to be that the position of greatest phonological strength is "initial position in the phonic group" (utterance-initial) or immediately following nasals (which generally assimilate to the following obstruent and thus block weakening -- e.g. [um$báso] but not [um$β̞áso] for "un vaso"). The adequate explanation for these weakening processes at hand (from Vulgar Latin to Spanish), then, is the explanation that has already been quoted from Nessly and credited by Nessly to Charles Pyle. This is the assumption that motivation for weakening is found in the reduction of articuatory effort in pronouncing non-prominent sounds. This would explain why the intervocalic obstruents (regardless of their positions relative to the syllable) have weakened while the same segments in utterance-initial position or in contrast with an immediately adjacent consonant maintain their original integrity.

Let us reiterate briefly the point I would like to emphasize. The historical rules showing Latin voiceless stops voicing intervocalically for Spanish, but not in word-initial position, or Latin voiced stops being susceptible to loss in varying degrees intervocalically but again stable word-initially, support any contention that what is observed here as a "weakening" of segments through a series of sequential steps leading to deletion is not an effort at weakeing or deletion in essence at all, but rather an attempt to reduce the articulatory prominence of sounds which occupy a structurally less significant position in utterances.

The significance of the example of Latin/Spanish stops is that it emphasizes that in the case of Spanish, at least, strong and weak position is not to be measured -- as attempted in Hooper's assessment -- by its position in the structure of the syllable. The synchronic fact of weakening of the Spanish stops in syllable-final position is, of

course amenable enough to Hooper's theory (since syllable-final position is defined as weak position). But historical (sapere/saber) or synchronic (lago/laɣo, soldado/soldaðo/soldao) weakening in syllable-initial position is not. The generalization that Hooper misses (if she is aware of it she doesn't formally capture it) is that all these cases of weakening -- syllable-final or intervocalic which is syllable-initial -- are quite unexceptionally word-internal. And with rapid speech phenomena (Hammond 1976) word-initial stops are also subject to this spreading weakening chain effect (e.g. va a darmelo/[baðármelo] "he's going to give me it", las damas/la[zð]amas, los gatos/lo[zɣ]atos). The generalization clearly is that stops in Spanish are subject to weakening in all but utterance-initial and post-nasal positions.

An even closer look at the synchronic syllable-final examples of weakening will lend further support to the Natural Phonology account of weakening chains. As I have been suggesting, the synchronic data from Spanish dialects is evidence that weakening processes involving consonants are not merely of the type considered by Nessly -- intervocalic -- but have a more generalized environment to be summarized as "structurally non-prominent". Although Spanish obstruents appear in syllable-final (structurally non-prominent in that the optimal syllable pattern for Spanish is CV) position in the orthography and in most careful speech, in rapid speech where pressures toward articulatory relaxation are greatest these obstruents are regularly weakened and even deleted. N.b. we can cite the following types of examples:

(16) CONSONANTAL REDUCTIONS IN RAPID SPEECH

ORTHOGRAPHY	PHONETIC REPRESENTATION		SPECIAL CUBAN REDUCTIONS	
	Weakening	Deletion		
septiembre	[se^{p}tyémbre]	[setyémbre]	[sektyémbre]	[se?tyémbre]
octubre	[o^{k}túβre]	[otúβre]		
absoluto	[aβsolúto]	[asolúto]	[aksolúto]	[a?solúto]
advertir	[aðβertír]	[aβertír]		

The most relevant observation in light of our present discussion is that there appear to be two alternate routes by which this weakening or reduction might be carried out by individual speakers. The first, as illustrated with (16), involves retaining a characteristic voicelessness but simply reducing the closure required for articulation of the stop. Reduced sonority in the reduced stop leads eventually to complete deletions (see Hooper 1973, page 140, for further discussion). For some American Spanish dialects, however, a second strategy to weaker articulation is regularly chosen (Hooper 1973, page 140). In these dialects the stop is simply replaced by a glide -- the weakest possible consonant. Here the weakening is manifested in voicing and the increased sonority resulting from this voicing makes the segment more vowel-like and therefore more stable in the syllable-final position. Total deletion of the segment is thus prevented.

(17) FURTHER CONSONANTAL REDUCTIONS

ORTHOGRAPHY	PHONETIC REPRESENTATION
	Weakening by Glide Substitution
afecto	[afé̯wto]
carácter	[karáwter]
satisfacción	[satisfaysyón]
actor	[awtór]

Two facts are highly significant about the dialectal variations shown in (17). First, as with all phonological phenomena which seem to result from processes of a "conspiratorial" nature, speakers have more than one apparent alternative route in Spanish to achieve the desirable phonological goal -- a reduced articulatory effort when pronouncing a segment of reduced structural significance. (I interpret "reduced structural significance" here to suggest those environments for which speakers of any given language sense they are entitled to expend less articulatory effort -- e.g. syllable-final and word-final positions in Spanish dialects.) And second, examples of the kind given for Spanish with (16) and (17) provide a modest degree of evidence which bears strongly on the question raised by Nessly as to whether such apparent weakening processes are in fact no more than cases of consonantal assimilations to the surrounding vowels. If we take careful note of what are the transparently obvious intermediate stages involved in alternations like the last form in (17) (actór, voiced to agtór, spirantized to aɣtór, and then vocalized to awtór), it then becomes evident that such steps in weakening do not involve assimilation (notice e.g. the important intermediate step which is the voicing of a consonant before another voiceless one). And the very existence, at least for some dialects, of the outputs of (17) reveal that an ultimate goal is not deletion but only reduction by any of several alternative means. This reiterates the conclusion of Nessly and Pyle: that the nature of the underlying principle behind surface manifestations of "weakening" is no more than reduced articulatory effort wherever it can be reasonably achieved.

NOTES

[1]My understanding of "weakening chains" and my interest in this topic have been advanced by informal discussions of the subject with Larry Nessly during the 1974 Linguistic Society of America Summer Meeting in Amherst. It is worth pointing out that Nessly quotes Richard Rhodes with originating the term "weakening chain" and Charles Pyle with having suggested the broader concept of "target chain" within a framework of Natural Phonology. Nessly also credits Pyle with inspiring the more sophisticated final notion (Nessly 1973, pages 472-3) of weakening chains as being distinct from conspiracies and universal rules. An original version of this chapter was written for presentation at the Third Hispanics Colloquium in Oswego, New York (August 1976). A revised and somewhat altered version is scheduled for presentation under the title of "Weakening Chains and the Histories of Some Spanish Consonants" at the 1976 Linguistic Society of America Annual Meeting in Philadelphia (December 1976).

[2]Foley specifies "strong" position here as the position between a stressed vowel and a syllabic /l/ or /n/. More generally, strong position here would seem to have more to do with syllable-initial position when occupied by the segments in question.

[3]Nessly cites an interesting example (borrowed from Chen) of a weakening process in a Northern Min (Fuzhou) dialect of Chinese which illustrates (1) that consonant weakening in intervocalic position is not strictly a European phenomenon and (2) that there is a definite interaction between such processes and a preferential weakening of obstruents from velars (weakest) to labials (strongest). The relevant facts are that in this dialect (1) intervocalic voiceless obstruents become fricatives if they are labials; (b) they become the sonorant [l] if they are dentals; and (c) they delete only if they are velars. See Nessly 1973, page 467, for the example and reference to Chen's original analysis.

[4]Notice that Hooper's scale of consonantal strengths does not capture this generalization that dental stops are weaker than labial or velar stops. She has not classified stops from strength according to point of articulation and d remains grouped with b and g as t is with p and k. The solution is suggested in the text of Chapter Nine (Hooper 1973), however.

[5]The distinction between mixed-color diphthongs and sonority-color diphthongs is made clear throughout Patricia Donegan Miller's work, particularly in Donegan Miller 1972a and Stampe 1972. Within Donegan Miller's theory, vowels have but three cardinal properties -- sonority, palatality, and labiality. Sonority (represented in [a]) is the syllabic and tone bearing property of vowels and is characterized by maximally open and minimally constricted vocal

tract; Palatality ([i]) and Labiality ([u]) are chromatic properties and are in turn characterized by minimally open and maximally constricted vowel tract.

[6]The theory that "strong" consonants are expected to strengthen in "strong" position but "weak" consonants are not (suggested by the fact that English t goes to ʔ in strong position while weaker p and k -- i.e. weaker by Foley's system -- are not permitted to shift) is again contradicted as a universal theory by the evidence from dialects of Spanish. Spanish glides (weakest segments among the consonants since they are stronger only than true vowels) will strengthen to obstruents in syllable-initial position (see Hooper 1973, page 138; and note also that Spanish consonants $\hat{y}$ and $\overset{w}{g}$ are derived historically as well as synchronically from the glides /y/ and /w/).

Appendix

APPENDIX PART I

TABLE OF MIAMI CUBAN INFORMANTS

INFORMANT	SEX	AGE	BIRTHPLACE	MIAMI/CUBA RESIDENCE (in years)	SPAN/ENG USAGE (percentages)
DC	Female	24	Havana	5 / 17	80/20 (Restricted)
LB	Female	21	Camagüey	11 / 9	40/60 (Excellent)
OV	Female	26	Havana	5 / 18	85/15 (Excellent)
AC	Female	17	Havana	10 / 5	30/70 (Restricted)
LR	Female	42	Havana	16 / 26	30/70 (Excellent)
HC	Female	50	Las Villas	6 / 40	100/0 Monolingual
AF	Male	48	Matanzas	7 / 41	100/0 Monolingual
CB	Male	25	Camagüey	6 / 13	30/70 (Restricted)
AV	Male	37	Havana	14 / 23	50/50 (Restricted)
RR	Male	47	Havana	14 / 33	80/20 (Restricted)
SA	Male	32	Oriente	6 / 17	25/75 (Restricted)
LC	Male	58	Havana	6 / 48	100/0 Monolingual

SUMMARY

Sex Distribution: Six female and six male informants.

Age Distribution: Range from 17 years to 58 years. No very young or very old informants. Six informants between 35 years and 60 years of age. Six informants between 17 years and 32 years of age.

Bilingualism: Three informants spoke excellent (nearly native) English (see extreme right-hand column above). Six informants had restricted command of English (hesitating speech, pronounced most English words with a heavy Spanish accent). Three informants were monolingual speakers of Spanish and used no English at all (with an exception of a few isolated and partially-nativized words). All informants spoke a native Cuban Spanish with full fluency and half (six) reported that they continue to employ Spanish for at least 80 percent of their daily work and social activities.

Miscellaneous: All informants had lived in Miami for at least five years, although three were currently residents of Gainesville and one of Jacksonville (Florida). The informants represent five distinct Cuban provinces, with seven naming Havana as their place of birth.

APPENDIX PART II

LINGUISTIC QUESTIONNAIRE

INTRODUCTORY STATEMENT

The following sentences, repetition items, and word lists comprise a linguistic questionnaire presented to each of the twelve informants who provided the Spanish data (Chapter Five) and loanword data (Chapter Seven) discussed in sections of this dissertation.

In addition to responding to the items on this questionnaire, each informant was also encouraged to provide several minutes of solicited "free conversation" which was obtained through presenting the informant with several leading questions (e.g. "Do you feel yourself to be more Cuban or more America?") or asking him to comment on a number of drawings or photographs. This questionnaire is in several respects similar to one prepared by Professor Bohdan Saciuk (University of Florida and Inter-American University of Puerto Rico) for use in his own more extensive studies of the native phonology of MCS. The author of this dissertation collaborated with Professor Saciuk of this project while serving as his graduate research assistant.

PART ONE: REPETITION ITEMS

Explanation

The informant was asked to read each of the sentences from one group (sentences on separate file cards) with normal and rapid delivery.

Group A Sentences

1. En realidad la actitud de Toni es algo foni.
2. El broder mío vive también en la sagüesera.
3. Vamos a West Palm Beach en el Winter para coger un tan.
4. Pacho dijo que estará avéilabol hasta las seis o las siete.
5. Seguro que María no está en nosin. Pero ¡qué sui es su abuelita!
6. Creo que él estará en probeichon mientras esté en la universidad.
7. Juan siempre hace espí cuando tiene mucho que estudiar.
8. ¡Qué cul está tu camisa sexy! ¡Qué nais está tu casita!
9. Me siento daun ahora y no quiero ir ni al parti ni al mitin con Juan
10. Ya ellos habían tomado el brei cuando llegaron el forman y el manager de la factoría.

Group B Sentences

1. Ella no es amiga mía porque siempre arma mucha troba.
2. El cosin mío fue a Casablanca Motors donde siempre tienen muchos specials.
3. Para mí un domingo es siempre un día bisi.
4. Carmen está tan diprés por todo lo que le pasó a Alejandro.
5. Después de mi tacita de café ya me siento op y salgo para mi apointment downtown.
6. La hermana mayor está singuel todavía y entonces Juanita no puede casarse.
7. Mi mamá siempre quiere ir de chopin al Mid-Way Mall u otro chopin center.
8. Favor de enviar mi Cash Card para que pueda beneficiarme del Servicio Bancario de la Luz de la Luna de los First State Banks.
9. Un garage donde se ofrecen servicio de Goderich, tune-ups, y juegos de frenos debe cobrar bastante.
10. Cuando ellos van donde hay playa siempre tienen mucho fon.

Group C Sentences

1. Confíe sus ahorros a Bankamigos de West Palm Beach.
2. Primero cogí un incomplí el el curso y después lo dropeé.
3. Mi día of es el sabado pero mi día bisi es siempre el domingo.
4. Siempre él está erli y to estás lei.
5. Dicen que Nixon está ronin otra vez para presidente.
6. ¿Dónde jangueaste el pansú?
7. El dijo que lo hizo por fon cuando fue a la playa y cogió un sonbern de madre.
8. Con las niñas el flaquito siempre es tan chai.

9. Fuimos a Hialeah Race Track por la expressway más moderna en todo el Dade County.

10. Este es el señor que tiene mucho pul en la administración y muchos amigos en la aduana.

PART TWO: RECOGNITION ITEMS

Explanation

The informant was instructed to pronounce each word from one of the lists below and to explain briefly what it means in current usage.

Group A List	Group B List	Group C List
achiquenearse	aproachear	bosear
bribar	buquear	failear
deitear	espelear	foldear
feiquear	flachear	janguear
frisear	guaxear (waxear)	postear
lonchear (lunchar)	meilear	rumear
rilaxear	ringuear	teipear
sainear	taipear	biliadora
vacunclinear	yoguear	jevísimo
subway	estepleador	standar (standard)
chiteador	blok	bridge
record	doyles	downtown
bloomer	kake	brasiere
clutch	likear	board
pantijós	spray	handicap
leiof	show	chitear

PART THREE: COMPLETION ITEMS

Explanation

The informant was asked to repeat each sentence, filling in the blank with any correct response.

Sentences

1. El State Department ahora quiere entrar en relaciones con (Cuba).

2. El caso de Vietnam ocupa un lugar preferente en el Congreso de los (Estados Unidos).

3. Kent, Chesterfield, y Kool son nombres de (cigarrillos).

4. Flagler, Hialeah Drive, y la Calle Ocho son calles en el centro

de la ciudad de (Miami).

5. Un amigo mío süinguió el bate tan duro que lo (rompió).
6. Para cachear un cheque es mejor ir a un (banco).
7. Esquipear, flonquear, cuitear, y dropear son todas palabras que tienen algo que ver con (el colegio).
8. Miguel es vendador de drogas y anoche fue bosteado por la (policía).
9. En McDonalds podemos comer un Big Mac, un apple pie, y _____.
10. El sacramento que se toma antes de la comunión se llama la (confesión).
11. En Cuba cien centavos son un (peso).
12. Otro nombre para grocery o supermercado es super (market).
13. Matamos puercos y vacas para comer la (carne).
14. Consuelo es nombre de (mujer).
15. En la televisión se dice que aunque se meta en el agua un Volkswagen no se (hunde).
16. En comparación con lo que hay en Miami, Harlem es un sitio bastante _____.
17. La discusión sobre la situación cubana siguió entre el Secretario Kissinger y el President (Ford).
18. No sabemos bien la situación en Cuba porque hay una falta de información en la prensa y en _____.
19. El número que viene antes de quince es (catorce).
20. El chocolate, el jamón, el ñame, y los french fries son cosas para (comer).
21. El pulover machea bien con los pantalones si los dos son del mismo (color).

22. Para los cristianos el redentor del hombre es (Cristo).
23. El flu, la difteria, y el tifus son (enfermedades).
24. Woolworths y Burdines son nombres de (tiendas).
25. Una tienda discount es una tienda donde venden las cosas a precios más _____.
26. Es más lenta la vida en el campo porque hay menos _____.
27. El cowboy americano siempre va montado en un (caballo).
28. En amor todo es _____.
29. Las señoras que lucen muy latinas tienen pelo de color (negro).
30. Un anarquista es uno que cree que no debemos tener un (gobierno).
31. La gente indígena de América son los (indios).
32. En los países pobres la falta de alimentación causa que muchos ninos (se mueran).
33. En vez de mentir, es mejor decir la (verdad).
34. La nariz y la boca son dos partes de la (cara).
35. Para ir a los drive-ins uno necesita tener un (carro).
36. Una mandolina, una trompeta, y un violín son (instrumentos musicales).
37. El órgano del cuerpo que bombea sangre es el (corazón).
38. Se hace un sandwich con dos pedazos de (pan).
39. Si uno no se siente bien, se dice que está mal o que está (enfermo).
40. Para endulzar el café se usa (azúcar).
41. Usamos hit y out y estrike cuando hablamos de (pelota).
42. Usamos round y nockout y punch cuando hablamos de (boxeo).
43. Se vende chiclet, vániti, y shampoo en _____.
44. Cuando uno trabaja horas extras en la factoría se dice que está

trabajando (overtime).

45. Porque iba demasiado rápido en mi carro, la policía me puso un (ticket).

PART FOUR: REPETITION ITEMS

Explanation

The informant was instructed to read off each string of words three times, as rapidly and as naturally as possible.

un boleto un francés un domingo un señor un chiste

el pomo el verde el médico el ñame el frente

ser padre ser bueno ser mejor dar jamón ser fino

absoluto adaptado eclipsar étnico septiembre concepción

un árbol enorme enamorado enumerar energía anzuelo antes un amigo un dependiente un dentista

APPENDIX PART III

SELECTED ENGLISH LOANWORDS IN MIAMI CUBAN SPNAISH

GENERAL STATEMENT

In Part Three of this dissertation loanword data from the Miami Cuban dialect of Spanish are reviewed as evidence for certain claims in Natural Phonology, especially for the hypothesis that foreign sounds are perceived by speakers of the target language (in this case Spanish) in terms of native underlying forms and for the related hypothesis that foreign sounds which approximate native segments derived by "processes" are lexicalized as appropriate native underlying forms and then derived by the relevant processes.

The following is a partial and yet representative inventory of English loanwords in this dialect which have served as a basis for the discussion of loanword phonology in Chapter Seven and which may provide a stimulus to further discussions as well. Forms marked with * represent single and perhaps unrepresentative pronunciations recorded on my tapes of MCS informants. Included in this listing are generally words and phrases in common usage among the speakers of MCS (I have attempted to avoid in as many cases as possible what might be a singular and perhaps uncharacteristic pronunciation) and the list as a whole is intended only to be partial and suggestive.

Note that although the same symbol l is used for both Spanish and English alveolar laterals, it must be kept in mind that in Spanish this sound is always palatalized but in English in word-final and pre-consonantal positions it is velarized. Also, it should be remarked that those cases of phonetic glottal stops occurring in Spanish forms were first called to my attention by Bohdan Saciuk (e.g. [wulwur?]).

TYPE ONE LOANWORDS

Foreign phonetic segments and sequences of segments are taken to be identical (even though they may not be precisely) to native underlying and phonetic segments and sequences and thus the word is both lexicalized and pronounced in its perceived phonetic shape. Examples:

ENGLISH PHONETIC FORM	ENGLISH ORTHOGRAPHY	SPANISH PHONEMIC FORM	SPANISH PHONETIC FORM
[kʰúl]	cool	/kul/	[kúl]
[hayəlíə]	Hialeah	/hayalia/	[hayalía]
[mayaémi]	Miami	/mayami/	[mayámi]
[sɛ́ksi]	sexy	/seksi/	[sɛ́ksi]
[ə́rli]	early	/erli/	[ɛ́rli]

TYPE TWO LOANWORDS

Foreign phonetic sequences are introduced into the native phonological system or native phonetic segments are introduced in those environments in which they do not normally occur. Examples:

ENGLISH PHONETIC FORM	ENGLISH ORTHOGRAPHY	SPANISH PHONEMIC FORM	SPANISH PHONETIC FORM
* [pʰaenaém]	Pan Am	/panam/	[panám]
* [mayaémi bíyč]	Miami Beach	/mayami biš/	[mayámi bíš]
[dʌ́bəlyu tʰíy gṙaént]	W. T. Grant	/dabalyuti grant/	[dáƀalyutí gránt]
[wÚlwəṙOs]	Woolworth's	/wulwurʔ/	[wúlwurʔ]
[ǰáeksənvIl]	Jacksonville	/ǰaksonbil/	[ǰáksõŋbil]
[ǰánsən]	Johnson	/ǰonson/	[ǰṍŋsõŋ]
[yÍdIš]	Yiddish	/ǰidiš/	[ǰíđiš]
[fə́ṙst fɛ́dəṙəl]	First Federal	/fuṙfederal/	[fUṙféđeral]
[wɛ́st pʰálm bíyč]	West Palm Beach	/wespambi/	[wEspambí]
[síyṙz]	Sears	/sirs/	[sírs]
[šák]	shock	/čoʔ/	[čóʔ]
[kʰáeš kʰáṙd]	cash card	/kaš kar/	[káš kár]
[Íŋkʰəm tʰaéks]	income tax	/inkontaks/	[ĩŋkõŋtáks]

TYPE THREE LOANWORDS

Foreign phonetic sequences are perceived as being either acceptable native phonemic forms or derived native sequences: in the former case the borrowing is lexicalized in its surface phonetic shape, while in the latter the speaker arrives at an acceptable underlying form through backwards application of native allophonic processes or application of dominant paradigmatic processes. In either case, surface phonetic renditions of these borrowings are achieved through subsequent forwards application of the native allophonic processes, wherever these are applicable. Examples:

ENGLISH PHONETIC FORM	ENGLISH ORTHOGRAPHY	SPANISH PHONEMIC FORM	SPANISH PHONETIC FORM
[fʌ́ni]	funny	/foni/	[fóni]
[nʌ́θIŋ]	nothing	/nosin/	[nósĩŋ]

ENGLISH PHONETIC FORM	ENGLISH ORTHOGRAPHY	SPANISH PHONEMIC FORM	SPANISH PHONETIC FORM
[swíyt]	sweet	/swi/	[swí]
[spíyd]	speed	/espid/	[espí]/[ehpí]
[t^{h}aén]	tan	/tan/	[táŋ]
[ṙíyzən]	reason	/rison/	[rísõŋ]
[haṙlәm]	Harlem	/harlen/	[hárlẽŋ]
[haembәṙgәṙ]	hamburger	/hambege/	[hambégE]
[p^{h}Úlowvәr]	pullover	/pulober/	[pulóβEr]
[Iŋkʌmplíyt]	incomplete	/inkompli/	[iŋkomplí]
[sawθwɛ́st]	southwest	/sa͡uswes/	[sauwé]
[no:θwɛ́st]	northwest	/norswes/	[norswé]
[hówmstɛd]	Homestead	/hamsted/	[hámstE]
[ɛ́kspɔ:t]	export	/ekspor/	[ékspor]
[p^{h}aenaém]	Pan Am	/panan/	[pãŋãŋ]/[panãŋ]
[jɛ́ṙәld fóṙd]	Gerald Ford	/yeral for/	[yéral fór]
[nuwǰ́әrzi]	New Jersey	/nuyersi/	[nuyérsi]
[nuwhәmpšIr]	New Hampshire	/nuhanšir/	[nuhãŋšir]
[t^{h}íkh^{h}ɛt]	ticket	/tike/	[tíkE]
[ówvәṙ t^{h}áym]	overtime	/obertayn/	[oβertãyŋ]
[míyDIŋ]	meeting	/mitin/	[mítĩŋ]
[dÍskawnt]	discount	/diskaun/	[dihkãũ]
[tṙ́әbәl]	trouble	/troba/	[tróβa]
[bÍziy]	busy	/bisi/	[bísi]
[dIpṙɛ́st]	depressed	/dipres/	[dipréh]
[šaemp^{h}úw]	shampoo	/čampu/	[čampú]
[šáp^{h}Iŋ]	shopping	/čopin/	[čópĩŋ]
[saén(d)wIč]	sandwich	/sanwiše/	[sãŋwiše]

ENGLISH PHONETIC FORM	ENGLISH ORTHOGRAPHY	SPANISH PHONEMIC FORM	SPANISH PHONETIC FORM
[bṙʌ́ðəṙ]	brother	/broder/	[bróɖer]
[léyt]	late	/lei̯/	[léi̯]
[pʰaéntsuwt]	pants suit	/pansu/	[páŋsu]
[sʌ́nbəṙn]	sunburn	/sonbur/	[sóŋbur]
[wɛ́stčɛstəṙ]	Westchester	/wesčester/	[gu̯Ečéhter]
[kʰéymaṙt]	K Mart	/kei̯mar/	[kéi̯mar]
[šáy]	shy	/čay/	[čáy]
[šów]	show	/čow/	[čów]
[bṙéyk]	break	/brei̯/	[bréi̯]
[súwpʰəṙmaṙkʰɛt]	supermarket	/supermarke/	[supermárkE]
[nÍksən]	Nixon	/nikson/	[níksõŋ]
[vówkswaegən]	Volkswagen	/boswagon/	[bóswaɡõŋ]
* [aépʰəl pʰáy]	apple pie	/apepai̯/	[apEpái̯]
[bIgmaék]	Big Mac	/bima/	[bimá]
[məkdánəlz]	McDonalds	/makdonol/	[makdónol]
[kʰiywɛ́st]	Key West	/kiwes/	[kiwés]
[flaégləṙ stéyšən]	Flagler Station	/flagler estečion/	[flágler estéšiõŋ]
* [kʰúwlz]	Kools	/kul/	[kú]
* [čɛ́stUṙfiyl(d)z]	Chesterfields	/čestefil/	[čehtefíl]
* [k'ʰɛ́nt]	Kent	/ken/	[ke]
[viyətnám]	Vietnam	/bietnan/	[bietnã́ŋ]
[stéyt dIpʰá:tmənt]	State Department	/este deparmen/	[ehtéy depármẽŋ]
[bɔ́ṙd]	board	/bor/	[bór]
[staéndəṙd]	standard	/estandar/	[ehtándar]
[ṙʌ́nIŋ]	running	/r̄onin/	[r̄ónĩ́ŋ]

ENGLISH PHONETIC FORM	ENGLISH ORTHOGRAPHY	SPANISH PHONEMIC FORM	SPANISH PHONETIC FORM
[pʰáentʰiyhowz]	pantyhose	/pantihos/	[pãŋtihós]
[ṙáwnd]	round	/r̄on/	[r̄õ̃ŋ]
[Ikspṙɛ́swey]	expressway	/espreswei̯/	[ehpréhwei̯]
[déyd kʰáwD̃i]	Dade County	/dei̯kau̯nti/	[dei̯kã̃u̯ⁿtI]
[háy skúwl]	high school	/hai̯skul/	[hai̯hkúl]
[pʰÚl]	pull	/pul/	[púl]
[fʌ́n]	fun	/fon/	[fõ̃ŋ]
[fɔ́:t lɔ́Dəṙdeyl]	Fort Lauderdale	/forlardel/	[forlárdel]
* [θaenksgÍvIŋ]	Thanksgiving	/sangibin/	[sãⁿɡ́ɓ̃ĩŋ]
[sÍŋgəl]	single	/singel/	[sĩ̃ŋgel]
[pṙowbéyšən]	probation	/probei̯čon/	[proɓéi̯čõŋ]
[əvéyləbəl]	available	/abei̯labol/	[aɓéi̯laɓol]
[ṙánəl(d) ṙéygən]	Ronald Reagan	/r̄onal r̄egen/	[r̄ónal r̄égE]
[sÍDi]	city	/siti/	[síti]
* [hʌ́dsən]	(Rock) Hudson	/hokson/	[hóksõŋ]
[wɔ́šIŋtʌn]	Washington	/wašinton/	[gu̯ásĩŋtõŋ]
* [šIkágo]	Chicago	/čikago/	[čikáɡ́o]
* [aéndṙuwz]	Andrews	/andru/	[áŋdru]
[saéksəfown]	saxophone	/saksafon/	[saksafõ̃ŋ]
[sIlvéyniə]	Sylvania	/silbani̯a/	[silbáni̯a]
[dáwntʰawn]	downtown	/dau̯ntau̯n/	[dau̯ntã̃u̯ŋ]
* [ṙówd áylən(d)]	Rhode Island	/r̄oai̯len/	[r̄oái̯len]

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BIOGRAPHICAL SKETCH

Peter C. Bjarkman enrolled in the Graduate School of the University of Florida in September 1972, after having served for over a year as Director of TESOL Programs and Principal of the American Division at the American School in Guayaquil, Ecuador.

Dr. Bjarkman has also been employed as an instructor of English at the Colegio Panamericano in Bucaramanga, Colombia, and has accumulated seven years experience as a secondary school teacher of English in Connecticut and Florida. He has recently been appointed Assistant Professor of English and Linguistics at George Mason University (the regional state university for Northern Virginia) where he will assume his new teaching duties September 1, 1976.

Dr. Bjarkman is married to the former Mary Anita Whitworth of Richmond, Virginia, and they have two children, a son Chris (born in 1968) and a daughter Kim (born in 1972). He was raised in Hartford, Connecticut, where he was born on May 19, 1941, and educated in the public schools of East Hartford. He earned his B.S. degree (1963) in Secondary Education (Magna Cum Laude) from the University of Hartford; subsequently he took the M.Ed. degree (1970) in Educational Administration also from the University of Hartford, as well as an M.A. degree (1972) in English from Trinity College. His master's thesis treated the novels and shorter fiction of John Updike. He is currently a member of the Linguistic Society of America, the Modern Language Association, and the National Council of Teachers of English. In

addition to linguistics, poetry, and literary criticism, the author's multifarious intersts include: following professional hockey, collecting books and articles on hockey, collecting turtles and reptiles, rock music, and jogging.

Professional papers include: "Toward a proper conception of processes in natural phonology" (published in the proceedings of the Chicago Linguistic Society Eleventh Regional Meeting, 1975); "Rule order and Stampe's natural phonology" (presented at the 1974 LSA Summer Meeting and reproduced by the Indiana University Linguistics Club); "The phonemic hypothesis and some related issues in natural phonology" (presented at the 1976 LSA Summer Meeting); "A review of A Synchronic Phonology of Mandarin Chinese by Chin-chuan Cheng" (Papers in Linguistics 8. 203-30); "Explanation in Stampean natural phonology" (presented at the Eleventh Regional Meeting of the Southeastern Conference on Linguistics, 1974); "Weakening chains and the histories of some Spanish consonants" (presented at the 1976 LSA Annual Meeting); "Sentimentalisms in Goldsmith revisited" (unpublished ms.); and "Updike and the retrospective narrator (unpublished ms.).

Most recently, Mr. Bjarkman has been invited by the editors of the journal Communication and Cognition to write the introductory paper on Natural Phonology for their special phonology issue to appear early in 1977 and later be released as an independent reader in phonology by Story-Scientia Publishers (Ghent, Belgium).

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality as a dissertation for the degree of Doctor of Philosophy.

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